

# BYZANTINISCHE ZEITSCHRIFT

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UNTER MITWIRKUNG ZAHLREICHER FACHGENOSSEN

MIT UNTERSTÜTZUNG DES THERIANOSFONDS  
DER KÖNIGL. BAYERISCHEN AKADEMIE DER WISSENSCHAFTEN

HERAUSGEgeben von

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## I. Abteilung.

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### The treatise *De administrando imperio*.

The treatise of the Emperor Constantine VII, known by the inappropriate title with which its first editor Meursius endowed it, *De administrando imperio*, has been in parts as diligently studied as any document of Byzantine literature. But it has been studied only in parts. The precious and unique notices bearing upon Slavonic and Hungarian history have been microscopically scrutinised by the curiosity of Slavonic and Hungarian investigators, eager to penetrate the darkness which envelopes the early fortunes of their races; and others have joined them in the search. The treatise lends itself to piecemeal treatment. The sections relating to the Croatians and Servians, Magyars and Patzinaks, the Iberians and their neighbours can be extracted and printed as independent documents.<sup>1)</sup> The result has been that the treatise has not been studied as a whole. Such a study is however indispensable. It is now universally recognised as a fundamental principle in historical work that philological criticism (literary and quellenkritisch) is the necessary preparation for a satisfactory use of authorities. Documents are not ready for the constructive operations of the historian till they have been submitted to the analytical operations of the philologist. An analysis of Constantine's treatise is all the more a desideratum, because its varied contents are so important to historical investigators in many fields of research. It would be impossible to acknowledge too gratefully the valuable material, serving provisionally as a commentary on the *De administrando imperio*, which Rambaud supplied in his great monograph on the life and times of

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1) For instance, the Slavonic by Šafarik, the South-slavonic by Rački, the Hungarian in the new publication of the Hungarian Academy which will be noticed below.

Constantine Porphyrogennetos.<sup>1)</sup> But the French historian has devoted only four pages to the consideration of the treatise as a whole. I hope that the study, which I offer now, of the composition of the work and its sources may not be unwelcome as a first attempt to supply what I have felt myself to be a serious want.<sup>2)</sup>

#### Summary.

- § 1. Author's division of contents
- § 2. The *κεφάλαια*
- § 3. Chronological data
- § 4. Evidence of patchwork: cc. 29—36 (Dalmatia)
- § 5. Evidence of patchwork: cc. 14—25 (Saracens)
- § 6. The formula *λοτίον δέ* (*δέ*)
- § 7. Sources: embassies &c
- § 8. South Italy (c. 27): source &c, The Narses story
- § 9. South Italy (c. 29). Relation to Vita Basilii
- § 10. Venice (cc. 27, 28): source
- § 11. Italy (c. 26): source. Liutprand's *Antapodosis*
- § 12. Dalmatia and the Southern Slavs (cc. 29—36): sources
- § 13. Hungarians and Patzinaks (cc. 37—40): sources &c
- § 14. Sarkel (c. 42). The Continuation of Theophanes
- § 15. The siege of Patrae (c. 49): source
- § 16. Chronological conspectus
- § 17. Logical defects of the treatise, and signs of incompleteness. Its value.

§ 1. While the Emperor Constantine doubtless availed himself largely of the help of secretaries and amanuenses in the composition of this treatise<sup>3)</sup>, there is not the smallest evidence to suggest that it

1) As Diehl truly said in 1899: *aujourd'hui encore chose vraiment extraordinaire en une matière que les récentes recherches ont presque renouvelée, ce livre, vieux de près de trente années, n'est point un livre vieilli* (*Études byzantines*, 1905, p. 27). We may repeat the remark to day, and the book is six years older.

2) I have not taken the trouble to look at G. Laskin's *Sochineniia Konstantina Bagrianorodnago o Themaki o Narodakh*, Moscow 1899, as it is abundantly evident from the long notice of S. Papadimitriu in B. Z. IX (1900) 515—23, that it is quite worthless and nothing is to be learned from it.

3) I will refer to it in future as *Adm.* The unfortunate name introduced by Meursius has been too long, and too widely, current to be discarded. *Περὶ ἑθῶν*, though incomparably better, does not cover the latter portion of the work. *Ad Romanum* would be most correct; but then there is another treatise which can claim the same title, in the Appendix to *De cerim.*, Bk. 1.

My references are throughout to the pages of Bekker's text (1840). What Bekker did was to collate the old text of Meursius (1611 and 1617) based on an inferior Palatine Ms. with the improved text of Banduri (Imp. Orient., vol. 1, 1711) based on the best existing Ms., Parisinus 2661, now 2009; to record the variants of these editions; and to add some corrections from the Parisinus itself.

is not his own work in the fullest sense of the word, in the details of execution as well as in the general design and arrangement. He speaks in his own name in the preface (pp. 65—67) and in other passages where his son is addressed (67—68, 90, 4—6, 182, 1—12, 213, 23—214, 2, 216, 12—18). We get the personal note too in an ironical reference to Romanus Lecapenus (241, 8 *τῆς βασιλείας οὐκ οἰδ' ὅπως εἰπεῖν ἐγκρατοῦς γενομένου*).<sup>1)</sup> We have no reason to question his personal responsibility for all parts of the treatise.

In the preface, p. 66, he explains its arrangement. He states that it falls into four divisions:

1<sup>o</sup> principles for dealing with the barbarian neighbours of the Empire, showing what peoples are dangerous and how they can be kept in check by raising up other peoples against them;

2<sup>o</sup> the unreasonable demands of the *εθνη* and how to meet them;

3<sup>o</sup> descriptive ethnology, history, and geography of the peoples surrounding the Empire; and on some passages (*τῶν ἐν τινι καιρῷ συμβεβηκότων*) between the Empire and various peoples<sup>2)</sup>;

4<sup>o</sup> some domestic innovations, and public events, within the Empire.

The treatise corresponds to this description of its contents, and the divisions between the four sections are marked by transitions in which the Emperor addresses his son. There are further indicated three subdivisions of the third Section which occupies nearly half the work: see p. 182 (c. 43) *περὶ μὲν τῶν βορείων Σκυθῶν ἵκανῶς σοι δεδήλωται — δεῖ δέ σε μηδὲ τὰ πρὸς ἀνίσχοντα ἥλιον ἀγνοεῖν*, and pp. 213—4 (c. 46) *ἴσθι δὲ καὶ κτλ.*

§ 2. Let us now tabulate the chapters (*κεφάλαια*) and see how they correspond to the author's divisions.

In Bekker's notes *vulgo* means Meursius + Banduri. For instance, the restoration of *Μορδίας* for *Μηδίας*, p. 166, 18, is due to Bekker. It is curious that Bekker did not prefix a word of preface explaining what he had done. The MSS. have been discussed by Vári in the Akadémiai Értéssítő of the Hungarian Academy, 72, 710—12, Dec. 1895. His article is not at hand, but from the notice of Pecz in B. Z. VI 590 I infer that he has not mentioned the Palatinus of Meursius. This exists as no. 126, ff. 2—129, of the Palatini in the Vatican. It was written by Antonius eparcha, when he was a boy, in 1509. See Stevenson, Codd. MSS. Pal. Graec. Bibl. Vat. (1885) p. 60.

1) Also in the Iberian narrative pp. 200—5.

2) This last might be considered an independent section, but the author seems to connect it more closely with section 3. The four divisions are indicated by *πρὸτα μὲν —, δεύτερα —, τέτταρα οὖτως —, and καὶ μετά ταῦτα —.*

## Table of Contents.

1°: how to manage the  $\xi\vartheta\nu\eta$  = pp. 67—81 = cc. 1—13, paragraphs 1 and 2;

2°: how to treat the demands of the  $\xi\vartheta\nu\eta$  = pp. 81—90 = rest of c. 13;

3°: ethnology, geography &c,

- A. of Saracens, Spain, Italy, Dalmatia, and northern  $\xi\vartheta\nu\eta$ ,<sup>1)</sup> = pp. 90—182 = cc. 14—42;
- B. of eastern  $\xi\vartheta\nu\eta$  = pp. 182—213 = cc. 43—46 (except last paragraph);
- C. on certain relations between the Empire and various  $\xi\vartheta\nu\eta$  = pp. 213—216 = cc. 46 *fin.* — 48 *med.*;

4<sup>o</sup>: internal innovations = pp. 216—270 = cc. 48 *med.* — 53.  
 This table shows at a glance that the Chapters do not correspond to the author's division of the material or reproduce his intention.  
 1) C. 13 is headed *περὶ τῶν πλησιαζόντων ἔθνῶν τοῖς Τούρκοις*, a description which applies only to the first paragraph. The second paragraph (p. 81, 13—15) concerns the hostility of the Patzinaks to the Hungarians. It is clear that these two notices ought to form one, or two, separate Chapters; it is absurd that they should be stuck on to the beginning of a Chapter which is occupied with the Second Section of the treatise. 2) And it is absurd, if the Chapters and their headings have any meaning, that this Second Division should have no heading at all. 3) It is similarly illogical that the transition from the 3<sup>rd</sup> to the 4<sup>th</sup> Section should be in the middle of a chapter (48).

We can hardly escape the conclusion that originally there was no division into numbered chapters. The division was based on short summaries or descriptions which were written in the margin, according to a common practice, to facilitate the perusal. But whether these marginal indices were added (as I should consider probable) in the original Ms., or not, they did not represent, and were not intended to determine, adequate subdivisions of the work. They were only a rough guide for the reader, and in many cases they are omitted where we should expect them. A further examination will make this clearer.

P. 79. At the end of c. 9, which is devoted to the Russians,

1) The divisions are of course incomplete and illogical: the Saracens should come under B and Italy with Dalmatia should have formed a distinct subdivision. The distinction of B from A seems to have been an afterthought. See below § 17.

comes the statement *ὅτι* of *Οὐέσοι δύνανται τοῖς Πατέριναιταις πολεμεῖν*. This is quite alien to what goes before, and if the chapters meant anything, should form a chapter by itself. That it was not separated is due to the fact that it was not distinguished by a marginal index.

Pp. 82 *sqq.* It is remarkable that no attempt was made to digest the Second Section of the treatise by marginal summaries. As three *ἄκταιροι αἰτήσεις* are discussed, it naturally falls into three parts (82, 6—84, 10; 84, 11—85, 21; 85, 22—90, 3), which could not fail to constitute three chapters, if chapters had been part of the design of the book.

Pp. 96 *sqq.* Cc. 21 und 22 ought, if the divisions were logical, to be broken up each into several chapters, on the analogy of cc. 15—20. This part of the work, which is largely derived from Theophanes, will be submitted to a closer examination presently (§ 5); but I will point out here how the heading of c. 21 betrays the nature of these soi-disant titles.<sup>1)</sup> *ἐκ τοῦ Χρονικοῦ Θεοφάνους*. *ἔτος ἀπὸ κτίσεως κόσμου* *εροα*. It may be observed that the description "from the Chronicle of Theophanes" does not apply to the greater part of the chapter; but I will not press this now, as I shall have to return to it. But "the year A. M. 6171" is not the date of any of the events which are *described* in the chapter. The event with which the chapter opens (*εἰσῆλθον* of *Μαρδαται* *εἰς τὸν Αἴβανον*) belongs to the year 6169 (Theoph. s. a.); and it is dated as *πρὸς τὴν τελευτὴν Μαβίον*. *Μαβίας* died in 6171; therefore the words *ἔτος ἀπὸ κτίσεως κτλ.* are obviously a marginal note to *τελευτὴν*.

P. 113. It is evident that the two last paragraphs of c. 25 should be distinct chapters, and that the description at the head of *κεφ. κέ*, "from the history of Theophanes" applies only to the first paragraph (pp. 110, 8—112, 5).

Pp. 121, 19—122, 23. All this Venetian matter is connected more closely with the following chapter than with the South-Italian affairs which occupy the main part of c. 27.

P. 217. The heading of c. 48 (*δέ ξητῶν ὅπως — ἐκ τῆς παρούσης μανθανέτω γραφῆς*) suggests by its form that it is a marginal addition.

P. 220. The heading of c. 50 ("concerning the Peloponnesian Slavs and the Mainotes") applies only to pp. 220, 21—224, 17, and ignores all the rest of the "chapter" (pp. 220, 22—233), which deals with various matters and ought to constitute a number of different *κεφάλαια*.

1) See too the titles of c. 17, 22 and 25 ("from Theophanes"); c. 48 ("29<sup>th</sup> Chapter of the Trullan Synod"); c. 16 ("from the canon of Stephanos").

Pp. 242—3. The last three paragraphs of c. 51 are not covered by the heading.

P. 214, c. 47. Here we have a different case. The title, unlike all the other titles, is categorical in form: *περὶ τῆς τῶν Κυπρίων μεταναστάσεως ἔχει ἡ ἴστορία τάδε*. The text begins *τῆς νήσου ἀλωθείσης*, although the island has not been mentioned before. It is evident that the sentence *περὶ τῆς — τάδε* is not a heading but part of the text.

This examination proves beyond question that the *κεφάλαια* do not represent a logical division of the contents, but are haphazard marginal indices.<sup>1)</sup> Of course, if this system of marginal indication of topics had been carried out completely, it might have served as the basis of a convenient capitular arrangement; but, as we have seen, it is sadly defective, and consequently the arrangement for which it is responsible only perplexes the reader and disfigures the construction of the work, helping to conceal the significance of the addresses to Romanus by which Constantine deliberately marked off not only the four chief Sections of the work, but the subdivisions of Section 3.

When I refer in this article to the numbered chapters, it is for the sake of convenience, and without prejudice to this conclusion.

§ 3. The composition of the treatise extended over some years. The terminus post quem is evidently July 15, 948, the date of the death of Romanus I, who is repeatedly spoken of as no longer alive (cp. p. 88). Cc. 27 and 29 were written in A. M. 6457, ind. 7 = A. D. 948—9 (pp. 120, 137), and c. 26 not later than A. D. 950 (p. 118, *Αωθαρίῳ τῷ νννὶ ὄντι Ἰταλίας φῆγμι*).<sup>2)</sup> On the other hand c. 45 was written in A. M. 6460, ind. 10 = A. D. 951—2 (p. 199). Rambaud draws the following conclusion: “Ainsi les 29 premiers chapitres au moins furent rédigés en 949 et 950; deux années s'écoulerent avant la rédaction du chapitre 45; et ce n'est guère que l'année suivante (953) que le livre parut à la lumière.”<sup>3)</sup> This statement suggests that, in Rambaud's conception, the first part of the book was composed in 949, and that the rest was gradually added during the next three years, or else the work was left aside and completed in 952.

1) One marginal index has been preserved *in the margin* of the Parisinus, at the beginning of c. 42; see Banduri's note, ed. Bonn p. 369, in ms. eadem manu ad marginem scribitur: *περιήγησις γεωγραφικὴ τῆς Σκυθικῆς γῆς*.

2) Lothar died on Nov. 22 950 (Dümmler, Otto der Große, p. 184, n. 2).

3) L'empire grec, p. 172. I do not see the object of adding 'et 950', since this year, the date of Lothar's death, is only a *limit*.

On the same principle Marczali infers<sup>1)</sup> as probable that the chapters on the Hungarians and Patzinaks, cc. 36—40, because they lie between c. 29 (A. D. 948—9) and c. 45 (A. D. 951—2), must have been written in A. D. 950, 951.

If this theory of the continuous composition of the work in the order which its sections occupy were correct, we could point to an interval of at least a year in which it was entirely intermittent. For we have in any case a definite limit of date for the composition of c. 30. We find there the following notice (p. 144, 7—10): *οὐ δὲ λοιποὶ Χρωβάτοι ἔμειναν πρὸς Φραγγίαν καὶ λέγονται ἀρτίως Βελοχρωβάτοι — ἔχοντες τὸν ἰδιον ἄρχοντα· ἵπκεινται δὲ Στρώ τῷ μεγάλῳ φηγῇ Φραγγίᾳ τῆς καὶ Σαξίας.* The meaning has been explained convincingly, and for the first time, by Westberg<sup>2)</sup>, who has shown that Constantine's *Βελοχρωβατία* includes Bohemia, Moravia, and the land of the Slovaks.<sup>3)</sup> The reference is therefore to the Bohemian realm, and the *ἰδιος ἄρχων* of the time was Boleslav I. But Boleslav was reduced by Otto the Great in summer (May or June) 950.<sup>4)</sup> Here then we have a new date for Adm. — July 950 as the upper limit for c. 30. The manner however in which Constantine speaks of the subjection of the Bohemian kingdom to Otto does not suggest that the words were written in consequence of an immediate announcement of the German king's success. We can say with probability that c. 30 was written *after* 950. There was therefore an interval of more than a year, probably not less than two years, between c. 30 and the notice in c. 29 which is dated 948—9.

If then the portions of the book were written in the order in

1) A magyar honfoglalás kútfói (publ. by the Hungarian Academy), 1900, p. 90: a magyarokról szóló fejezetek a kettő közé esnek és így némi valószínűség szól a mellett hogy ezeket 950—951-ben írta.

2) Ibrahim's-Ibn-Is'kub's Reisebericht über die Slawenlande aus dem Jahre 965 (in the Zapiski of the St. Petersburg Academy, ser. VIII, cl. hist.-phil., III 4 1898) pp. 97—101. These pages are valuable for the study of Constantine's notices of Great Moravia and White Croatia. One of the points is that the old Lech kingdom had ceased to exist before the time of Constantine, perhaps extinguished by Sviatopluk.

3) Schafarik had already seen that Moravia and Bohemia were implied (Slawische Altertümer II p. 244), but he failed to advance to Westberg's logical inference. Westberg equates the Northern Servia with Galicia + Little Poland = Quellengebiet of Dniester and Vistula. I cannot accept this White Servia. There can be little doubt, I think (with Roesler and Jagić), that *Βόικι τόπος* (Adm. 152) is Boio-haemum.

4) See Dümmler op. cit. pp. 180—1. Boleslav had been independent since A. D. 936.

which they respectively stand, we have to suppose that the work was interrupted in the middle of the part about Dalmatia and not resumed for two years or more. But is the underlying assumption tenable? There are obviously two other possibilities. The treatise might have been sketched out as a whole and the greater part of it written in 948—9, but some sections, whether few or many, might have been inserted during the next few years. Or the articles on the various subjects treated might have been prepared, independently of one another, and “pigeon-holed”, during the years 949—52, and not arranged in their final order before 952 or later. I will show hereafter (§ 11), from chronological data, that c. 26 was composed not before the 8<sup>th</sup> indiction, and therefore *after* c. 27, which, as we have seen, is dated to the 7<sup>th</sup> indiction. This result would enable us to reject the theory of consecutive composition. But independently of definite chronological data, an examination of the treatise reveals facts, hitherto ignored, which can only be explained by one of the two alternative hypotheses, a rehandling and expansion of a preliminary sketch, or compilation from a collection of notices, written with a view to incorporation in the treatise but irrespectively of the order in which they were subsequently to appear. I proceed to set forth the grounds for this conclusion.

§ 4. It has been shown above that while c. 29 was written in the 7<sup>th</sup> indiction, 948—9, c. 30 was not written before the 9<sup>th</sup>, 950—1. These chronological data are confirmed by other internal evidence which shows that these two chapters could not conceivably have been composed as a whole by the same author at the same time. The foundation of Spalato is recorded three times<sup>1)</sup>; but the decisive proof that the two chapters were written independently of each other is the duplicate narrative of the Avar capture of Salona, pp. 141, 15—143, 20 = 126, 8—128, 2. An inspection shows at once that we have here to do with two reproductions of the same original document, made by the author at different times.<sup>2)</sup>

Now the following chapters, 31—36, which treat of the Slavs of Dalmatia and Servia, are closely connected with c. 29 and were composed contemporaneously. This is practically proved by the fact that in c. 29 the writer refers the reader in anticipation to cc. 31—36. He says (128, 7): *ὅτι ἀπὸ τῆς βασιλείας Ἡρακλείου τοῦ βασιλέως Ρωμαίων*,

1) Pp. 125, 21, 137, 15, 141, 11; it is mentioned again in connexion with the Croatians p. 149, 5.

2) See below § 12.

καθ' ὃν μέλλει τρόπον ὁγθῆσεσθαι ἐν τῇ τῶν Χρωβάτων καὶ Σέρβων συγγραφῇ, πᾶσα ἡ Δελματία καὶ τὰ περὶ αὐτὴν ἔθνη οἷον Χρωβάτοι Σέρβοι Ζαχλοῦμοι Τερβούνισται Καναλεῖται Διοκλητιανοὶ καὶ Ἀρεντανοὶ καὶ οἱ Παγανοὶ προσαγορευόμενοι\*.<sup>1)</sup> Now the συγγραφή of the Croatians Servians &c is cc. 31—36. It is justifiable to infer that in writing this passage Constantine had prepared and was about to compose the notices in 31—36.

It follows that cc. 29, 31—36<sup>2)</sup> form a consecutive text, and that c. 30 is a later addition, composed independently and containing partly new and partly old material. In writing 30 the author had forgotten that he had already related the capture of Salona in 29, and is equally oblivious of the fact that the coming of the Croatians and their conquest of the Avars are related in 31. These two duplications suggest both that the insertion of 30 was considerably later, and that the work never enjoyed a final revision.<sup>3)</sup>

It may be added that the portion contained in 30 differs *in form* from 28. It is to be observed that the notices throughout the greater part of the treatise are introduced by the formula *ιστέον ὅτι* or simply *ὅτι*, — a point which will be considered more fully below (§ 6). Now in 29 as in 31—36 these formulae are used, as normally, but not in 30. Whereas 30 is introduced by a preface such as does not occur elsewhere except at the beginning of a main division or subdivision of the work: *εἰ πᾶσιν ἡ γνῶσις καλόν, — ἵνα διπλοῦν ἐπανακολουθῇ τὸ καλόν* (p. 140).<sup>4)</sup> Thus formally also, 30 is an interruption; 31 follows naturally on to 29.

We thus see that the theory of consecutive composition is untenable. But there are more proofs.

### § 5. The 3<sup>rd</sup> Section opens with an historical sketch of the Cali-

1) The predicate has fallen out of the text, and has been supplied in Banduri's translation in a sense exactly the reverse of that which is required. This has been pointed out by Grot, *Zur Kritik einer Stelle des Constantinus Porphyrogenitus*, Arch. f. slav. Phil. 5 (1881) 392. He suggests *δουλικῶς εἰσιν ὑποτεταγμένοι τῷ βασιλεῖ Ρωμαίων*. Rather: *προσαγορευόμενοι <δουλικῶς ἡσαν τῷ βασιλεῖ τῶν Ρωμαίων ὑποτεταγμένοι>*. The omission was ex homoeoteleuto.

2) The *Zusammengehörigkeit* of these chapters, as distinct from 30, is further illustrated by the fact that in them the relation of the Slavonic settlements to Heraclius is prominent (his name occurs in ten contexts), whereas in 30 where the Croatian conquest is also noticed there is no reference to him.

3) Further, in writing 31, he knew nothing of the baptism of the Croatians in the 9<sup>th</sup> century which he records in 30. See below § 12.

4) It differs from the transitions between Sections in not being expressly addressed to Romanus.

phate, and an analysis of this portion (pp. 90—106), which is mainly derived from Theophanes, will afford us further insight into the process by which the treatise was compiled. We must examine it in detail, and for this purpose I will break it up into divisions distinguished by letters of the alphabet.

*a* = pp. 90, 13—92, 7 (= c. 14), on Mohammad, is taken almost word for word from George Monachus, II 697—699 and 706 (ed. De Boor), who depends here mainly on Theophanes A. M. 6122 (De B. 333, 14—334, 22).<sup>1)</sup>

*b* = p. 92 (= c. 15), origin and character of the Fatimites. Source unknown. Here, as in *a*, Mohammad is called *Μονχούμετ* (as in George Mon.); whereas in the parts derived from Theophanes the form is *Μονάμεθ*. The two forms are equated, p. 93, 9.

*c* = p. 93 (= c. 16), the Hijra. The title of the *κεφάλαιον* evidently consists of two marginal notes: [α] ἐκ τοῦ κανόνος ὃν ἐθεμάτισεν Στέφανος ὁ μαθηματικὸς περὶ τῆς τῶν Σαρακηνῶν ἔξοδου. [β] ἐν ποίῳ χρόνῳ τῆς τοῦ κόσμου συστάσεως ἐγένετο καὶ τίς ἦν τότε ὁ βασιλεὺς Ῥωμαίων. The second is the regular marginal index; while the first is a note to the words τὸ θεμάτιν τῶν αὐτῶν Σαρακηνῶν, supplying the source. It is to be observed that the date is given not in the era used by Theophanes, but in the Constantinopolitan reckoning.

*d* = pp. 93, 14—94, 15 (= c. 17), on Abubekr and the Mohammadan doctrines, is copied from Theophanes A. M. 6122, with the exception of two lines, p. 94, 1—3, which mark a transition in the transcription. Under this year, à propos of the death of Mohammad, Theophanes gives an account of Mohammadanism. The middle part of this account had already appeared in *a*, where it was taken not from the original but from George Monachus. Here the initial and final parts are copied, and are separated from each other by the two lines just mentioned. Thus:

Adm. 93,14—94,1 = Theoph. De Boor 333, 1—13

Adm. 94, 1—3

Adm. 94, 3—15 = Theoph. De Boor 334, 17—27.

But although *a* and *d* supplement each other, in general, in regard to the original, they repeat each other at one point. The final portion of the passage of Theophanes, which is copied fully in *d*, is reproduced briefly (from George) in *a* (91, 20—92, 2). Moreover one

1) The sentence καὶ ἐδίδαξεν — εἰσέγεται, 91, 21—92, 1, is only found in Coislinianus 305, which represents the first form of George's work: cp. De Boor, Praefatio to his ed. of George pp. LXVIII—LXX.

statement of the chronographer receives different interpretations in *a* and *d*:

*Theoph.* 134, 16: *καὶ οὗτως ἐκ γυναικῶν <ἢ φῆμη> ἤλθεν εἰς ἄνδρας, πρῶτον Ἀβουβάχαρον, ὃν καὶ διάδοχον κατέλιπεν.*

*a* 91, 17 (= George Mon.): *προηλθε τὸ φεῦδος τῆς ἀπάτης καὶ εἰς ἄνδρα φύλαρχον τοῦνομα Βουβάχαρ. ἢ ούν γυνὴ θανοῦσα καὶ τοῦτον διάδοχον καὶ κληρονόμον καταλείψασα τῶν ἑαυτῆς, ἐγένετο [sc. Βουβάχαρ] περιφανῆς καὶ ἄγαν ὑπερούσιος.*

*d* 94, 2: *πρῶτος ούν Ἀβουβάχαρ ἤκολούθησεν αὐτὸν καὶ προφήτην ἐκήρυξεν, διὸ καὶ διάδοχον αὐτὸν κατέλιπεν.*

It is evident that while the writer of *d* rightly referred *κατέλιπεν* to Mohammad, the author of *a*, i. e. George, imagined that the subject of the verb was *Χαδίγα*.

*e* = pp. 94, 18—96, 6 (cc. 18—20), caliphates of Abubekr, Omar and Othman, — brief notices derived from Theophanes:

94, 18—21 = Theoph. A. M. 6124, 6125

95, 3—14 = " " " 6127<sup>1)</sup>

95, 16—17 = " " " 6139

95, 19—22 = " " " 6140

95, 22—96, 2 = " " " 6145

At the end of the passage, the caliphate of Moāwiya is noticed. The expedition against Constantinople is mentioned (Theoph. A. M. 6165), and it is added: *ἐλυμήνατο τίν τε Ἔφεσον καὶ Ἀλικαρνασσὸν καὶ Σμύρναν καὶ τὰς λοιπὰς πόλεις Ἰωνίας*. Theoph. says nothing of Ephesus and Halicarnassus, but for Smyrna see *sub* A. M. 6164.

*f* = pp. 96, 9—97, 10 (c. 21), (1) the Mardaites, and the conclusion of peace between Moāwiya and Constantine IV, (2) the struggle between Moāwiya and Ali. Source, Theophanes:

97, 9—97, 2 = Theoph. A. M. 6169

97, 2—10 cp. Theoph. A. M. 6147, 6148, 6151.

*g* = pp. 97, 11—98, 1 (c. 21), the Maurophoroi, and Saracen occupation of Spain. This follows on to the last words of *f* — *ἐξράτησε δὲ ἡ αὐτοῦ γενεὰ ἐτη ο'* — and is to be compared with Theoph. A. M. 6240, 6241. The passage presents difficulties and must be more carefully examined.

*καὶ μετ' αὐτὸν [sc. Μαβίαν] ἔξηλθον οἱ λεγόμενοι Μανροφόροι ἀπὸ Περσίδος, οἱ κρατοῦντες ἐώς τῆς σήμερον, καὶ ἐπολέμησαν τὴν γενεὰν τοῦ Μαβίου καὶ ἡγάνισαν αὐτήν, ἔσφαξαν δὲ καὶ Μαρονάμ*

1) Of the capture of Jerusalem, the text of Theophanes gives *παρέλαβεν αὐτὴν λόγῳ*. Constantine gives (falsely) *δόλῳ* (not noticed by De Boor).

τὴν κεφαλὴν αὐτῶν. ὑπελείφθησαν δὲ δλίγοι τοῦ Μαβίου, καὶ ἐδιώχθησαν παρὰ τῶν Μαυροφόρων ἐώς τῆς Ἀφρικῆς μετὰ καὶ ἐνὸς ἐκγόνου τοῦ Μαβίου. ὁ δὲ αὐτὸς ἐκγονος τοῦ Μαβίου μετ' δλίγων τινῶν διεπέρασεν εἰς τὴν Ἰσπανίαν ἐν ταῖς ἡμέραις Ἰουστινιανοῦ τοῦ ἀνιστράτου, οὐχὶ δὲ τοῦ Πωγωνάτου. τοῦτο δὲ παρὰ τοῖς ἡμετέροις ἴστορικοις οὐ γέγραπται· ἀφ' οὐ γάρ παρελήφθη ἡ μεγάλη Ῥώμη παρὰ τῶν Γότθων, ἥρξατο ἀκρωτηριάζεσθαι τὰ Ῥωμαϊκὰ πράγματα, καὶ οὐδεὶς τῶν ἴστορικῶν τῶν τῆς Ἰσπανίας μερῶν ἐποιήσατο μνείαν οὔτε τῆς γενεᾶς τοῦ Μαβίου. ἔχει δὲ τοῦ μακαρίου Θεοφάνους ἡ ἴστορια οὕτως.

In the text, as it stands, there is a glaring contradiction. The writer, priding himself on his knowledge of the origin of the Omayyad dynasty in Spain, emphatically states that no Greek historian has recorded it; and then adds "such is the relation of Theophanes"! We cannot credit him with this absurdity.

As a matter of fact Theophanes has recorded the crossing to Spain sub a. 6241. Let us compare his narrative with Constantine's.

De Boor 424, 11 ἐκινήθησαν ἐκ τῶν ἀνατολικωτέρων μερῶν τῆς Περσίδος λαοὶ οἱ λεγόμενοι Χωροσανῖται Μαυροφόροι κατὰ τοῦ Μαρονάμ καὶ πάσης τῆς συγγενείας κτλ.

425, 13 καταδιώκεται Μαρονάμ ὑπὸ τῶν Μαυροφόρων καὶ καταληφθεὶς ὑπ' αὐτῶν κτείνεται.

426, 1 οἱ δὲ περισωθέντες υἱοί τε καὶ συγγενεῖς τοῦ Μαρονάμ ἐλθόντες ἀπ' Αλγύπτου εἰς Ἀφρικὴν κάκειθεν ἀντιπεράσαντες τὸ διορίζον μεταξὺ Λιβύης καὶ Εὐρώπης τῆς κατὰ τὸν Ὦκεανὸν στενῆς θαλάσσης, τὸ λεγόμενον Σέπται, τὴν τῆς Εὐρώπης Σπανικὴν ὥκησαν μέχρι τοῦτο τὸν χρόνον, ἔχοντές τινας προκατοικήσαντας αὐτόθι τῶν ἀπὸ Μανίου διὰ πλοὸς ἐκριφέντων ἐκεῖσε, συγγενεῖς αὐτῶν ὄντας καὶ τῆς αὐτοῦ θρησκείας.

The differences between the two accounts are fundamental, and there is no resemblance to warrant the assumption that Constantine was acquainted with the narrative of Theophanes. Constantine has confused two different things: the conquest of Spain by Tarik in A. D. 711 (in the reign, as he says, of Justinian) and the arrival of Abd ar-Rahman (ὁ ἐκγονος τοῦ Μαβίου) in A. D. 755, who inaugurated the Spanish dynasty of the Omayyads. In consequence of this confusion, he antedates the death of Marwan II and rise of the Abbāsids by forty years. And this mistake explains how it was that he overlooked the account of Theophanes. The emphatic assertion that "none of our historians" records these facts undoubtedly implies that he sought for a notice of them in Theophanes. He did not find it, because he

sought under the reign of Justinian. Theophanes places the event in its right chronological setting. The difference between the two accounts is further shown by the fact that, while Constantine has a clear grasp of the importance of one particular descendant of Moāwiya (sc. Abd ar-Rahman), Theophanes only speaks generally of sons and kinsmen of Marwan.

Hence it is quite clear that the sentence *ἔχει δὲ τὸν μ. Θεοφ. ἡ* *ἰστορία οὐτως* does not refer to what precedes. It seems to be a marginal note which has got into the text (with addition of *δέ*), and to refer to what follows.

*h* = p. 98, 1—16 (c. 21), the successors of Moāwiya. Source, Theophanes, A. M. 6171, 6175; and, for death of Constantine (with the erroneous addition *ὁ νῖστος τὸν Παγανάτον*), A. M. 6177.

*i* = pp. 98, 17—102, 16 (c. 21), Moāwiya as general and caliph, and the siege of Constantinople in A. D. 717. Source unknown. Here Moāwiya is introduced afresh, as if he had never been mentioned before. The destruction of the colossus of Rhodes is recorded at greater length, and the struggle with Ali (which had been noticed in *f*) is told in another and fuller form.

*k* = pp. 102, 20—106, 19 (c. 22), succession of the caliphs: Abd al-Malik and Justinian II, Valid, conquest of Africa and Spain, the successors of Valid. Source, chiefly Theophanes.

The text begins here abruptly: *αὐτη ἔστιν ἀρχὴ τῆς βασιλείας αὐτοῦ καὶ μετὰ ταῦτα ἔξεβλήθη κτλ.* The title of the *κεφάλαιον* explains who is meant by *αὐτοῦ*:

*ἐκ τοῦ χρονογράφου τοῦ Θεοφάνους περὶ τῶν αὐτῶν καὶ περὶ Μαρβίου καὶ τῆς γενεᾶς αὐτοῦ διεπέρασεν ἐν Ἰσπανίᾳ. Ρωμαῖον βασιλεὺς Ἰουστῖνος [sic] ὁ φινότυμητος.*

It is clear that we have here two distinct notes. (*α*) *ἐκ τοῦ — Ἰσπανίᾳ* is the marginal index, stating the subject of the text (103, 1 — 105, 2). But *περὶ τῶν αὐτῶν* is unintelligible; it could hardly mean *περὶ τῶν Ἀράβων*. I suspect that it is corrupt and that *⟨Μαρβ⟩αιτῶν* should be restored. (*β*) *Ρωμαῖον — φινότυμητος* is a marginal note, added to explain *αὐτοῦ*.

But the text, as it stands, evidently implies that *αὐτη ἔστιν ἀρχὴ κτλ.* was immediately preceded by a sentence in which Justinian was mentioned. In other words, it follows on immediately to p. 98, 16 (*καὶ ἐβασίλευσεν ἀντ' αὐτοῦ Ἰουστῖνιανὸς ὁ νῖστος αὐτοῦ*), and explains the distinction between his first reign and his later restoration. This is further made clear by the sentence which follows (103, 1): *τούτῳ τῷ ἔτει ἀποστέλλει Ἀβιμέλεχ πρὸς Ἰουστῖνιανόν κτλ.* In what year?

We have to go back to 98, 16 to discover: the year in which Constantine IV died and Justinian succeeded. Thus in order to explain the text we have to assume the absence of the portion which I have designated as *i* (= pp. 98, 17—102, 16).

We may now compare the text of *k* with Theophanes. p. 103, 1—17 = Theoph. A. M. 6178, De B. 363, 6—20. Then follows *τῷ δ' αὐτῷ ἔτει εἰσελθὼν δὲ βασιλεὺς εἰς Ἀρμενίαν κτλ.* This however does not belong to the same but to the following year (A. M. 6179). A passage must have fallen out of the text, corresponding to some of the notices in Theophanes, De B. 363, 21—32 (*τῷ δ' αὐτῷ ἔτει κτλ.*). pp. 103, 22—104, 5, see Theoph. A. M. 6190, 6187. pp. 104, 5—11 = Theoph. A. M. 6197.

At this point the series of notices derived from Theophanes is interrupted by a passage on the conquest of Spain pp. 104, 11—105, 2. The statement, previously made in *g* as to the arrival of Abd ar-Rahman is repeated, and a notice of the conquest of Crete in the reign of Michael II is added.

P. 105, 3 = Th. 6207; 105, 4 = Th. 6208; 105, 7 = Th. 6209; 105, 8 = Th. 6212; 105, 10 = Th. 6216; [Valid is omitted between Isam and Marwan, Th. 6234]; 105, 11 = Th. 6235; 105, 12 = Th. 6241; 105, 14 = Th. 6267; [Musa is omitted before Harun, Th. 6276]; 105, 15 = Th. 6278.

The next sentence is incomplete:

*ἐν τούτῳ τῷ χρόνῳ, ἥγουν τῆς τῶν Ρωμαίων ἀρχῆς Ελεήνης καὶ Κωνσταντος, ἔτος ἀπὸ κτίσεως κόσμου ,σοπή' [6288]. τῷ δ' αὐτῷ ἔτει Ἀράν — τέθνηκεν κτλ.*

The death of Harun fell in the reign of Nicephorus, in A. M. 6301 (στα'), see Theoph. Therefore a passage must have fallen out of the text in which were recorded events belonging to the years A. M. 6288 and 6301. It cannot be insignificant that not only is the A. M. given (the only case in this series of records), but it is related to the Imperial sovereigns. This suggests at once that the event recorded had to do with Roman and not with Saracenic history. Now if we turn up Theophanes under the two years in question we find that in both there are notices relating to the same event. In A. M. 6288 Constantine VI married Theodote, and the abbot Plato broke off communion with the Patriarch Tarasios; in 6301 Theodore, Plato, and the Studites broke off communion with the Patriarch Nicephorus διὰ Ἰωσὴφ ὡς παρανόμως στεφανώσαντα Κωνσταντίνον καὶ Θεοδότην. This coincidence suggests the inference that a notice of this affair originally stood in the text and has been omitted.

The rest of the text, to 106, 12 is transcribed from Th. A. M. 6301. Then follows a formal statement that the preceding canon of the succession of caliphs is taken from Theophanes, who is described as the *μητρόθειος* of Constantine VII.

But we have not yet quite done either with the Saracens or with Theophanes.

*l* = pp. 106, 22—110, 5 (cc. 23, 24), on Spain, and the names Iberia and Hispania (citations from Charax, Athenaeus, Parthenios &c).

*m* = pp. 110, 8—113, 5 (c. 25), the occupation of Spain by the Vandals and Visigoths. A transcript from Theophanes. It is introduced abruptly, beginning *τούτῳ τῷ έτει*, without any indication as to what year is meant. There was a marginal note (title of c. 25) *ἐκ τῆς ἱστορίας τοῦ ὁσίου Θεοφάνους τῆς Σιγγρανῆς*.

*n* = pp. 113, 6—114, 16 (c. 25), the Saracen *ἀμερούμενοις* (caliphs) and *ἀμηραῖ*.

It is obvious from the preceding analysis that the portion of the work relating to the Saracen powers (cc. 14—25) was not composed continuously or according to a single plan. It presents, as we have seen, the clearest traces of patchwork. Not to speak of repetitions, some of which might be explained as due to carelessness or inadvertence, there are two distinct and independent accounts of Moāwiya; and the section *i* intervenes between *h* and *k*, which closely belong to one another, in such a way as to render the beginning of *k* unintelligible.

It is also remarkable that in the account of Spain (*l + m*) there is no mention of its conquest by the Saracens. We should expect to find here the notices of it which appear in *g* and *k*.

Mere literary awkwardness (although it is a factor) is insufficient to explain this extraordinary arrangement. It can only be explained as the result of the method by which the material was prepared; in fact, it gives us a glimpse into Constantine's literary workship. We are driven to forming a hypothesis of the following kind.

The material which Constantine had, in the first place collected for the Saracen section of his treatise consisted of *a*, *b*, *i* and *n*. This first series is disconnected and independent, and is homogenous in character with the rest of the work (e. g. in the absence of any formal chronology). Subsequently the Emperor conceived the idea of introducing a brief chronicle of the caliphs. This idea was suggested by his interest in the Chronography of his *μητρόθειος* Theophanes; he does not carry the succession down beyond the beginning of the

ninth century where Theophanes comes to an end. Accordingly he compiled, or directed a secretary to compile, a second series of notices, chronological in form, and transcribed or abridged from the work of Theophanes. This second series consists of *d*, *e*, *f*, *h*, *k*.<sup>1)</sup> But Theophanes omitted to record the Hijra<sup>2)</sup>, and this omission was repaired by prefixing *c*, derived *ἐκ τοῦ κανόνος δν ἐθεμάτισεν Στέφανος ὁ μαθηματικός*.<sup>3)</sup> The Annus Mundi in this notice was in the Constantinopolitan reckoning; and the compiler has sought to facilitate, as it were, the juncture between *c* and *d* by explaining the *τούτῳ τῷ ἔτει*, with which the Theophanes-extract in *d* begins, in terms of the same era: *τούτῳ τῷ ἔτει, ἥγουν ,5918'* (p. 93, 14). Theophanes himself would have described it as *σοκβ'*.

The compiler had now to combine series 2 with series 1. This was done in a purely mechanical fashion, and badly at that, without any attempt at working the two sets of material into an ordered whole. There were two places at which the two series concurred; the notices of Abubekr and the doctrines of Islam in *a* and *d*; and the accounts of Moāwiya in *i* and *f*. The compiler ignored the former collision entirely. He took account of the latter only from a chronological point of view, namely so far as to see that it would be unsuitable for *i* to precede *a—e*. But instead of placing *i* either immediately before *f* or immediately before *h*, he inserted it, most stupidly and awkwardly, after *h*. The general principle which the compiler followed was to insert the chronological history, derived from Theophanes, between *ab* which treated *περὶ γενεαλογίας καὶ ἐθῶν* and *n* which explained the contemporary condition of the Saracen states. The only problem which he considered was how to deal with *i*, which belonged to the chronological history, and he solved it with a striking want of dexterity.

But there was yet a third series of notices pertaining to Spain: *l* (geographical), *m* (Visigothic conquest, from Theophanes), *g + k\** (Saracen conquest).<sup>4)</sup> The obvious thing to do was to place this series after the general history of the Caliphate, and accordingly the compiler has done so in the case of *l* and *m*. It would have been much better if he had been content to allow *g + k\** to follow *m*, for it is *g + k\** which justifies and explains the insertion of *lm* between *k* and *n*.

1) But perhaps *f* belonged to series 1: see below § 6.

2) Two of the inferior MSS. of Theophanes add at the end of A. M. 6113 (De Boor p. 306), *τούτῳ τῷ ἔτει Μάμεδ ἐφάνη ἀμηρεύσας ἦτη θ'*.

3) I will return to this notice below.

4) *k\** = portion of *k* relating to conquest of Spain.

But he preferred to introduce  $g + k^*$  into the context of the chronological history. His object in doing so was twofold: to bring the Spanish Omayyads into direct connexion with Moāwiya and to emphasize the right chronology. Hence he inserted  $g$  in the (Theophanes) account of Moāwiya, and  $k^*$  in the notice of the reign of Justinian II.

In stating such a precise hypothesis I may seem to push the limits of enquiry too far, and I am fully conscious that there is a line beyond which it is vain to seek to penetrate into the secrets of a literary *officina*. My intention has only been to illustrate the general proposition (which I consider that I have proved) that *a hypothesis of this kind* is necessary to explain the text of cc. 14—25.

The hypothesis is compatible either with the supposition that there were two redactions of the treatise as a whole, that in the first redaction the Saracen portion consisted of *abin* (series 1), and that the conflation with series 2 (+ series 3) was made for the second redaction; or with the supposition that there was only one redaction (not earlier than A. D. 952) and that the patchwork belongs entirely to the original process of preparation. The second supposition seems to me to be the one which we must accept. I can find no evidence whatever to *necessitate* the view that there were two redactions of the treatise as a whole; and I will afterwards give reasons for believing that the work was never completed. On the other hand, we can see that the nature of the work implied a collecting of miscellaneous material extending over some years, and we can understand how the idea of the contents may have grown in the Emperor's mind. In planning Section 3 of his treatise he may at first have contemplated almost entirely information derived orally from contemporaries, and the extracts from older, literary sources may have been due to an afterthought.

To avoid an interruption of the argument I postponed the consideration of the *Σεμάτιον* of Stephanos, which has some historical interest for the tenth century. Before this century we hear nothing of Stephanos of Alexandria or his horoscope of the Saracens; this notice in *Adm.* is the earliest. The import of his horoscope was that Sept. 3, Thursday, was a day fraught with fate for the Saracen empire. Constantine says nothing as to an anticipated fulfilment of the presage; he refers to it only in connexion with the astrologer's date of the exodus. But in the chronicles of Leo Grammaticus (or rather Pseudo-Leo) and George Cedrenus<sup>1)</sup> we can discover why his attention was

1) Leo, p. 152, Cedr. I 717. It may be asserted that Pseudo-Leo presents the *original* prediction of Stephanos, as preserved in a seventh century chronicle.

drawn to the *κανών* and *θεμάτιον* of Stephanos. In both these works the horoscope is interpreted. The power of the Saracens is to last 309 years. But before it is extinguished there is to be a period of *ἀκαταστασία* and misfortune. As to the length, however, of this second period the two chronicles differ. In Pseudo-Leo it is determined as 27 years, thus giving a total duration of 336 years to the Saracen empire; in Cedrenus it is determined as 56, giving a total of 365. We are thus in presence of the interesting fact that in the tenth century prophecies were current of an approaching dissolution of the Saracen empire, and that as the fatal year passed without fulfilment a new term was substituted.

But the period of prosperity remained a fixed quantity, 309 years; in other words, when the horoscope of Stephanos was resuscitated, the year 931 (622 + 309) was past, and the year 958 (931 + 27) had not yet come. But what determined the year 931 and the period 309? There was no collapse or sign of collapse in the events of that year; on the contrary, the Saracens achieved greater successes than the Romans.<sup>1)</sup> The reason lies in the nature of the horoscope. The fatal day was to be Sept. 3, but it was also to be Thursday. In A. D. 622 Sept. 3 fell on Wednesday, in A. D. 623 on Thursday. The number 308 is a multiple of the cycle 28 (in which the procession of correspondences between weekdays and monthdays recurs); hence 931, the 308<sup>th</sup> year from 623, was fixed on, as not only distinguished by the fatal Thursday = Sept. 3, but as cyclically corresponding. The next year of the same description was 959; we must infer that this form of the *θεμάτιον* originated in the interval. But the period of adversity ends in 958, not as we might expect in 959. The total number of 336 years (= 28 × 12) is calculated here from 622, not from 623.<sup>2)</sup> In 958 Sept. 3 fell on Wednesday, as in the year of the exodus. Perhaps students of astrology will be able to explain the reason of this difference. But it is clear that in the reign of Constantine, between 931 and 958, superstitious Romans congratulated themselves that the rival power had just passed its zenith and was

But unless astrological students can show that it would have been *according to rule* to fix on 308 (= 28 × 11) years, there would be no probability in such a view.

1) Cp. Vasiljev, *Vizantia i Araby, za vremia makedonskoi dinastii 232 sqq.*

2) The text of Adm. implies, but does not make clear, the distinction of the dates of the exodus and the horoscope: "The Saracens went forth Sept. 3, A. M. 6130 (= 622 A. D.), but their horoscope was drawn Sept. 3, Thursday (sc. A. M. 6131)". So Cedrenus gives the A. M. of the horoscope as *σάβ*. The text of Pseudo-Leo has *σάτ*, but *α* has probably fallen out before *ἀνά*.

destined to complete collapse within their own lifetimes. The reference to the *θεμάτιον* in this treatise, before 953 as we may assume, illustrates the fact that it was then talked about, though the Emperor does not condescend to particularize the tenor of the prophecy.

The fatal year, 958, came and passed, and men's hopes were disappointed. But such prophecies have a phoenix existence; they are falsified and renewed. The brilliant victories of Nicephorus Phocas and John Tzimisces supplied a good argument for a new edition of the horoscope. The period of prosperity was not altered, but the period of adversity was lengthened from 27 to 56 years, thus postponing the date to 987 A. D. Here again the 28 cycle is the determining factor. But in this case the calculation is from 623, and Sept. 3 falls in 987 on Thursday.  $987 = 623 + 364$  ( $= 28 \times 13$ ). This horoscope had the additional recommendation that the whole period from 622 amounted to 365 years, the number of days in the year, — a point to which attention is called in Cedrenus, with reference to the Kedar prophecy of Isaiah (XXI 16).

We can be quite sure that the *θεμάτιον* of Stephanos was mentioned in a chronicle older than the tenth century. The very existence of the notice in the late chroniclers guarantees such an origin. And if so, there can be hardly much doubt that it comes from the lost, or one of the lost chronicles which served as a source of Theophanes for the seventh century. But in copying the record, two 10<sup>th</sup> century chronographers altered the original prophecy into the versions of it which were current respectively when they wrote. Pseudo-Leo (Cod. Par. 854) reproduces a chronicle which must have been originally written close to 958 (probably before it, for after it the chronicler would hardly have altered the text of the document which he copied). Cedrenus reproduces apparently a later redaction of the same chronicle, made before 987 and containing the version of the horoscope which was current in the early years of Basil II. But the redactor added a remark *suo marte*. *λείπει ούν ἔως τῆς —, ἐλαφρῶς ἐθεμάτισεν* δάστρονόμος Στέφανος· ἀλλ' ὡς οἷμαι λεπτὸν παχὺ ἐλαθεν ἐκεῖνον.

There is an unfortunate lacuna after *τῆς*<sup>1)</sup>, but it is clear that the author of this observation wrote before 987, and did not himself alter the text of his source. In the history of this chronography there were therefore at least three stages before A. D. 987: (a) the text

1) *συμπληρώσεως* has been rightly supplied by Patzig, who has used the *θεμάτιον* passages of Pseudo-Leo and Cedrenus for the theory propounded in his article: *Leo Grammaticus und seine Sippe*, B. Z. III (1894) 495—6.

reproduced by Pseudo-Leo, written in the neighbourhood of 958, (b) the text in which the *θεμάτιον* was altered, (c) the text written by a sceptic, not long before 987, and reproduced by George Cedrenus.

It is clear that we come here into close quarters with the question which revolves round the mysterious chronicler whom one is tempted to call "der ewige Logothet". Some thought that he had been run to earth when Vasilievski announced the identity 'ot uachala do kontsa' of the Bulgarian translation which professes to be the work of Symeon metaphrastes et logothetes with the chronicle of Leo Grammaticus.<sup>1)</sup> This solution became untenable when it was shown that the anonymous chronicle of Cod. Par. 854 is not the work of Leo, since the latter part of it is far from being identical with the *χρονογραφία τῶν νέων βασιλέων* contained in Cod. Par. 1711 under Leo's name.<sup>2)</sup> The *anonymus* of Cod. Par. 854 was then entered for the place which Leo was forced to vacate, but in another and authoritative quarter Theodosius of Melitene<sup>3)</sup> has been considered a more likely candidate.<sup>4)</sup> The real truth probably is that the original work of the Logothete has not been preserved at all in a perfectly pure and uncontaminated form. But I am not going to enter into the question. There are only two points I wish to emphasize. The name of the Logothete, the original part of whose work was the history *τῶν νέων βασιλέων*, was certainly Symeon. This is proved, not I think necessarily by the superscriptions in the MSS. which might have been due to a confusion with the contemporary (but not demonstrably identical) Symeon Metaphrastes, but by the epitaph of Symeon the Logothete on the death of Stephanos son of Romanus I, preserved in Cod. Par. 1277 and published by Vasilievski.<sup>5)</sup> Here the name is quite independent of the chronicles, and no one can doubt that the sympathetic author is

1) *Khronik Logotheta na slavianskom i grecheskom*, Viz. Vrem. 2 (1892), 120.

2) Shestakov, *Parizhskaya rukopis khroniki Simeona Logotheta*, Viz. Vrem. 4 (1897), 167—88.

3) As to his date, let me call attention to a notice (which might pass unobserved) unearthed by Vasilievski (ib. 135 n. 3) from Becker's *Anecdota Graeca* III 1465, which points to A. D. 1120 as a lower limit. — Whatever be the relations of Leo, Theodosius &c, it is at least certain that our particular MS. of Leo, Par. 1711, is inferior for the later part of the Logothete's work to that of Theodosius. A good illustration is its omission of the passage *ἀρετῆς γὰρ εἰς ἔχον — ταπεινὸν δὲ τὸ φρόνημα*, a characterisation of Romanus I which is unmistakably from the pen of the Logothete. This is not one of the numerous cases of ex homoeoteleuto.

4) See De Boor, *Weiteres zur Chronik des Logotheten*, B. Z. X (1901), 89.

5) Viz. Vrem. 3 (1896), 574—8.

the same as the historian who has shown so undisguisedly his devotion to Romanus Lecapenus. Stephanos died in 963, just after the accession of Nicephorus Phocas.<sup>1)</sup> According to the ordinary view, it was at the beginning of this reign that Symeon wrote or completed his chronicle. But if so, it is almost inconceivable that he should not have referred to the death of Stephanos. We find at the close of his chronicle references to other events which happened after the accession of Nicephorus. The only explanation can be that he intended to continue his chronicle to a later date than 948, as indeed is implied in one passage; unless we accept Vasilievski's theory that the short portion on the *αὐτοκρατορία* of Constantine between 944 and 948 is not from his hand, but an addition made by Leo Grammaticus.<sup>2)</sup> Vasilievski based this view on (1) the circumstance that this portion is absent in the Bulgarian version, where the notice of the death of Romanus immediately follows the account of his deposition, and (2) on the expression *πληρωθεῖσα παρὰ Λέοντος γραμματικοῦ* in Cod. Par. 1711. It is unfortunate that Vasilievski has not told us whether the Bulgarian version also omits the notice of the duration of Constantine's whole reign<sup>3)</sup>, which must have been written after his death. I am not fully convinced, but in any case — and here I come to my second point — there is nothing to prevent our holding that the chronicle as a whole was written during the last ten years of the reign of Constantine. Its tendency would have rendered it impossible to make it public while he was alive, and a similar respect might have made the author keep it back during the reign of Romanus II. Then he might have made one or two additions shortly after the accession of Nicephorus and given it to the world in 963 before the death of Stephanos. This is conjecture, but what I would illustrate is that, whatever view we hold as to the close of the chronicle of Symeon, it is open to us to consider probable that its earlier portion was written before 958, and that the *θεμάτιον*, as it appears in Cod. Par. 854 (Pseudo-Leo), stood in the author's original work.<sup>4)</sup>

1) Cedrenus II 346, Zonaras III 482, 495.

2) Viz. Vrem. 2, 188 (cp. 99) and 4, 576.

3) Cont. Georg. 874 (ed. Bonn) = Leo Gramm. 288. Vasilievski ought to have added that, on his theory, Leo must have copied his addition straight from Cont. Theoph., and thus the words *ἐν τῷ προηγούμενῷ* (sic leg.) *ἔξιγγίσει ἐκθήσουσαι* (Leo 329) would be due to the author of Cont. Theoph. Bk. VI, in which work the promise (436) is fulfilled (438).

4) I am curious to know whether the *θεμάτιον* appears in the Bulgarian translation, and if so, in what form. Though Vasilievski asserted unreservedly

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§ 6. Attention was drawn above to the formula *ἰστέον ὅτι* or *ὅτι* by which the notices in Adm. are regularly introduced. The exceptions are as follows. (1) Section 2, where the formula is inappropriate as the section consists of direct admonitions to Romanus. (2) It is not used at the beginning of the main divisions: Sect. 1, p. 68; Sect. 3, A p. 90, B p. 182, C p. 214. Sect. A p. 216 is the exception. (3) Extracts and citations: Stephanus, p. 93; the Theophanes passages, pp. 93—106 (one exception, p. 96), and p. 110; the collection of citations on Spain pp. 106—110; extract from the acts of the Trullan synod, p. 215. Here too comes the list of horses supplied by the Peloponnesus, p. 243, 13—244, 2, obviously copied from an official report. (It is awkwardly separated from the preceding notice, to which it belongs, by a capitular division.) (4) The paragraph beginning *πρώτη ἡ παρὰ τῶν Χαξάρων* is only an apparent case, for it belongs closely to what goes before, from which it has been improperly disjoined by the introduction of *κεφάλαιον μ'.* There are three other cases: the *διήγησις περὶ τοῦ θέματος Δελματίας* (c. 30) which has already been discussed; the account of the repulse of the Slavs from Patrae (c. 49); and the story of Cherson (c. 53). But the two last narratives, concerning long past events, are not original compositions of Constantine but extracts from older works.<sup>1)</sup> They would thus come under the category of (3). The Dalmatian chapter is of a different nature, but we have seen that it was “a subsequent insertion” not contemplated in the compilation of cc. 29 and 31.

In view of these facts, it seems almost justifiable to infer that according to the original plan all the notices which were “pigeon-holed” for use in the treatise were prefaced by the formula *ἰστέον ὅτι* (*ὅτι*), with the exception of the cases noted in (1) and (2). All this material had been arranged in its proper order, when the Emperor decided to introduce other matter, consisting mainly of extracts from books. These insertions can be distinguished by the absence of the usual formula.<sup>2)</sup>

There are two exceptions, which perhaps may be said to prove the rule. (1) One passage from Theophanes in the Saracen portion is prefaced by *ἰστέον ὅτι*. I assumed above, naturally, that this passage (*f*) belonged to the rest of the Theophanes series, but, in

its identity with Pseudo-Leo, I infer from an observation of his own Viz. Vrem. 2, 120, that his collation of the two texts was very far from complete.

1) For the Cherson narrative see Garnett, Eng. Hist. Review 12 (1897), 100—5. For the attack on Patrae see below § 15.

2) *ἰστέον ὅτι* is also significant in the *De Cerimoniis*, as I will show elsewhere.

view of what has just been said, it seems possible that *f* should be separated from series 2 and placed in series 1. The original series which included an extract (*a*) from George Monachus may have included also an extract from Theophanes. (2) The notice of the invention of Greek fire by Callinicus of Heliopolis, with which Section 4 opens (p. 216) is taken from Theophanes<sup>1</sup>), *πῦρ θαλάσσιον* being replaced by *τὸ διὰ τῶν σιφάνων ἐκφερόμενον πῦρ*. (In Section 2 the legend that the secret of this explosive was revealed to Constantine the Great by an angel is related, — a legend invented for the purpose of investing the secret with sacrosanctity. Constantine VII was perhaps conscious of the inconsistency, for while he adopts *κατεσκεύασεν* "manufactured" from Theophanes, he omits the chronicler's addition *καὶ οὐτως οἱ Ρωμαῖοι — τὸ θαλάσσιον πῦρ εὗρον*.)

§ 7. The collection of material seems to have extended over at least three or four years (between 948—952). The portions which are most valuable for us (on the Russians, Hungarians, Patzinaks, Dalmatia &c) are those which were derived mainly, if not wholly, from oral sources. It would be very interesting to know how and in what circumstances all these items of information were taken down. We are here in contact with what may be called the intelligence bureau of the Byzantine government. Since the days of Justinian<sup>2</sup>) it was one of the principles of that government to collect all the information it could obtain concerning the social and political condition and relations of the surrounding barbarian states, for the practical purpose of guiding its own diplomacy. The opportunities for collecting such information were supplied by the embassies which went and came. We may conjecture that the accounts of Constantine (e. g. of Russian commerce) depend not directly on the communications of merchants or travellers, but on the reports of Roman ambassadors or on enquiries made from foreign envoys. Reports of embassies were almost an institution; and if Priscus wrote his famous description of the mission to Attila in the capacity of a historian rather than as a member of the embassy, the relation which Peter the Patrician drew up of his mission to Chosroes was undoubtedly official, in the first

1) A. M. 6165, De B. p. 354. The same notice occurs in (Pseudo-) Leo Gramm. 160 — Cedrenus I 765 — Mosquensis 406 (Muralt) p. 613. But is it there derived from Theophanes? The addition of *ἀρχηθε* makes me suspect that it was taken directly from the lost chronicle which was the chief source of Theophanes for the 7<sup>th</sup> century.

2) See Diehl's excellent chapter, L'œuvre diplomatique, in his Justinien (1901).

instance, though he was permitted, or directed, to publish it as a sort of semi-official "bluebook". Its official character can be inferred from the fact that it was written *not in the literary but in the spoken language*<sup>1)</sup>, a fact which we ought to place side by side with Constantine's use of the vulgar tongue in the *De administrando imperio*. Menander's account of the embassy to the Turks in the reign of Justin II was doubtless based on an official report of Zemarchos. Here<sup>2)</sup>, as in so much else, the Venetian republic learned from her former mistress; the famous *Relationi* of her ambassadors must have been originally suggested by the East-Roman practice.

It would seem however that these official records of the Foreign Office at Constantinople were not preserved throughout with sufficient care. Constantine VII, who took as much interest as Justinian himself in the diplomatic management of the "barbarians", caused the well-known *ἐκλογαὶ περὶ πρεσβειῶν* to be compiled, containing in its two sections historical accounts of the embassies of foreign peoples (*ἔθνους*) to the Romans and of the Romans to foreign peoples.<sup>3)</sup> But it consists of extracts not from official records but from historical writers. Peter's embassy to Chosroes in A. D. 562 is described, not from his own report, but from Menander. Can we avoid the conclusion that many records of the sixth century had been destroyed through carelessness, or perhaps by the accidents of fire?

But many of the notices which make Constantine's work so valuable were gathered in the tenth century in Constantine's own lifetime, — some of them, we may probably conjecture, for the purpose of being included in this treatise. It must be remembered that such information, before it reached the Emperor or his collaborators, had passed through the medium of interpreters — a fact which may explain some errors. An interpreter was a necessary adjunct to the staff of a Roman embassy, and there was a regular corps of *ἐρμηνευταὶ* at Constantinople, one of the seven *εἰδη ἀξιωμάτων* which in the 10<sup>th</sup> century were under the command of the *λογοθέτης τοῦ δρόμου*.<sup>4)</sup> Two interpreters for Armenian are mentioned in our treatise.<sup>5)</sup>

1) Menander, 12 (F. H. G. IV 217). He emphasizes the fulness of detail in Peter's account (ad fin., p. 218).

2) Liutprand's history of his mission to Nicephorus is a formal *Relatio* to the Ottos. Byzantine influence is evident.

3) Critically edited by De Boor as vol. I of Constantine's *Excerpta historica*.

4) *De ceremoniis*, II 52 p. 718.

5) *Adm. c. 43*, p. 184 *Θεοδάρου τοῦ τῶν Ἀρμενίων ἐρμηνευτοῦ*, p. 190 *Κριτῆν* *ἐρμηνέα*.

Arabic interpreters were in constant requisition<sup>1)</sup>; and for communication with the princes of the west not only were Latin interpreters required<sup>2)</sup>, but men who could draft imperial letters in Latin.<sup>3)</sup> For Slavonic there was no difficulty; it was easy to get from Macedonia or Bulgaria men who could with little trouble understand the language spoken at Kiev. Negotiations with the Russians<sup>4)</sup> were, we may presume, from the very beginning conducted in Slavonic and not in Norse; and it may be observed that, taking Thomsen's analysis of the names of the rapids of the Dniepr, the Slavonic are on the whole less seriously corrupted than the Norse, and the Greek interpretations seem to be intended as translations of the Slavonic names.<sup>5)</sup>

In the time of Constantine, it may be legitimately conjectured that for political intercourse with the long-established kingdom of the Khazars, which had given two Empresses to Byzantium (though the memory of the Iconoclast's consort does not incline Constantine VII to treat it with indulgence), there were interpreters for its own language; there would have been no difficulty in obtaining a supply of suitable persons from Cherson, where it is recorded that the missionary Constantine (Cyril) studied the Khazaric tongue.<sup>6)</sup> But what about the newer comers, such as the Patzinaks and Hungarians? Did the Logothete of the Course add to his staff interpreters for their languages? Considering the importance of the relations of the Empire with these peoples in the time of Constantine, it is difficult to see how special interpreters could have been dispensed with. In this

1) The successors of the ἐρμηνευταῖ for Persian, who are mentioned in the document from Justinian's reign (by Peter? cp. Krumbacher, G. B. L.<sup>2</sup>, 289) preserved in the *De cer.* II c. 89 p. 404, 18.

2) Anna, Alex. X 11 (p. 94 ed. Reifferscheid) ἐνα τῶν τὴν Λατινικὴν διάλεκτον μεθερμηνευόντων.

3) E. g. the famous communication of Michael II to Lewis the Pious; or the letters of Alexius I to the abbots of Monte Cassino, edited by Trinchera, *Syll. membranarum graecarum* (1865) nn. 61, 62, 66, 86.

4) For the negotiation of the treaties of 911 and 945 (the first of which, he wishes to prove, was only ratified by the second) see Dimitriu, *K voprosu o dogovorakh Russkikh s Grekami*, Viz. *Vrem.* 2, 539 *sqq.*

5) In the case of the first fall it is stated that both names have the same meaning (p. 75, 19). In the case of the fourth (whether Thomsen is right or not in explaining *Νεασήτ* as originally meaning "the insatiable"), it seems certain that the explanation διότι φοιενούσιν οἱ πελεκάνοι (p. 76, 20) refers to the (assumed) meaning of the Slavonic name. Compare also the sixth (p. 77, 13): *Σκλαβινιστὶ δὲ Βερούτζη, δὲ έστι βράσμα νεροῦ* (cp. *Βράτη*), where Thomsen interprets the Norse name *Λεάντι* as "laughing" (hlaejandi).

6) *Translatio Clementis* c. 6.

connexion, I would point out as remarkable that we find the laws or customs of the Patzinaks called *τὰ ζάκανα αὐτῶν* (p. 73, 20). In the same way, the Hungarians, in raising their newly elected chieftain on a shield, are said to follow *τὸ τῶν Χαζάρων ἔθος καὶ ζάκανον* (p. 170, 15: this is of course derived from a Hungarian, not a Khazaric source). In both cases *νόμος* would be appropriate; why is the Slavonic word (законъ) employed? Again, the chiefs of the Hungarians are called by the Slavonic appellation of *βοέβοδοι* (pp. 168—9 *passim*).<sup>1)</sup> If Constantine's notices were derived from Patzinaks and Hungarians through Patzinak-Greek and Hungarian-Greek interpreters, why does the Slavonic come in? My first thought was that they pointed to the inference that Slavs were employed for interpreting these languages. But this supposition is obviously insufficient. For such interpreters would have been able to express such simple terms in Greek without resorting to their own tongue. The truth, I suspect, may be that among these peoples, who were in constant intercourse with their neighbours the Bulgarians and the Eastern Slavs, the Slavonic language was a sort of lingua franca, so far at least as that a certain number of Slavonic words passed current among the non-Slavonic peoples of the Danube and Dniepr regions. In converse with foreigners, Patzinaks and Hungarians would be apt to use such words (even in talking their own tongues to an interpreter), and this would explain their appearance in the treatise of Constantine.<sup>2)</sup>

We might expect to find that the Emperor had sought information about "Scythia" directly or indirectly from the Khazars with whom the Empire was on very good terms; he could have got information from them, for instance, about Black Bulgaria. But not only do there seem to be no traces of Khazaric sources, but there is no description

1) Since the text was written, I see that Marczali refers to this fact (*A magyar honfoglalás kútföi*, p. 98) as having been noticed by Szabó, and rightly observes that it does not imply Slavonic sources. But I cannot agree that it is explained by saying that "a byzanczi nyelv már akkor telítve volt szláv elemekkel"; for why then do we find these words only in this particular context? We must deprecate any revival of the view that Constantine belonged to a dynasty of Slavonic origin ("tán szláv származású"); Basil I was of Armenian descent.

2) On the same principle that in translating, say, a Modern Greek official document into German, any French terms which occurred in the original would be retained untranslated. — In regard to the use of *βοέβοδος*, it is to be observed that a distinction is intended between the pre-Arpadian Chiefs (*βοέβοδοι*) and the post-Arpadian rulers (*ἄρχοντες*). *Ἄρχων* was the title used in official communications (*De Cerim. II* 48, p. 691).

of Khazaria itself, an omission which is distinctly remarkable. The references to the Khazars in Sect. 3 are all incidental.<sup>1)</sup>

The communications between Constantinople and vassals of the Empire, like Venice, must have been frequent; with Iberia, in the reign of Constantine, they seem to have been unintermittent. Rambaud remarks:

“Pour l’Arménie comme pour le Caucase, Constantine VII, dans sa studieuse retraite du Grand Palais, était en fort bonne situation pour être bien informé des affaires importantes. Ces renseignements, il put les demander à d’innombrables émigrés, bannis, aventuriers arméniens qui fuyaient devant les persécutations musulmanes ou cherchaient fortune sur les terres de l’Empire.”<sup>2)</sup>

Without questioning the possibility that recourse may have been had to such informants, it seems probable, as I have already said, that the sources were in the main of a more official kind.<sup>3)</sup> Rambaud properly refers to the visits of Armenian princes to Constantinople during the reigns of Romanus and Constantine, and to the Byzantine agents who were kept permanently in the Armenian states.<sup>4)</sup>

In examining Constantine’s sources of information, we have mainly to consider Section 3. In Sections 1 and 2 the author is expounding diplomatic principles, and almost the only passage which calls for comment is the description of the Russian trade-route to Constantinople, which is out of place where it is, and ought to have been inserted in Sect. 3. All that we can say about it is that the information may well have been derived either from Igor’s envoys who came to Constantinople to negotiate a treaty in 944, or collected by the envoys of Constantine who returned with them to Kiev to con-

1) It may be mentioned here that Westberg (op. cit. 134) explains Σαμβατάς which Constantine gives as a name of Kiev (75) as = Sabbath, and conjectures Jewish-Khazaric influence. The same name was given to the river Don. — It may be noted that Νεμογαρδάς in the same passage of Constantine is obviously a textual corruption of Νεβογαρδάς (“legendum Novogardia”, Banduri) due to the facility of confusing β and μ in tenth century MSS.

2) L’Empire grec, p. 495.

3) The section on Adranutzin (c. 46) is of course based entirely on strictly official information. Two reports, ἀναφοραί, of the Patrician Constans, are mentioned p. 211, and the Imperial instructions are quoted (p. 209, 4–14). So in c. 45 chrysobulls of Romanus and Constantine himself. These chapters are chiefly devoted to the history of Constantine’s own time. It is clear that in 951–2 he was very much occupied with the Armenian question.

4) Ib. 496.

clude it.<sup>1)</sup> Nor does Section 4, dealing chiefly with administrative arrangements of the author's time and his father's, present material that comes within our present scope. Here the Emperor had abundance of official data; the horse-levy of the Peloponnesus is copied straight from an official document (p. 243). The notices of Cherson and the Slavonic attack on Patrae are exceptions (see above § 6, below § 15).<sup>2)</sup>

The question of sources then is limited with few exceptions to Section 3. The first portion of this, relating to the Saracens, I have already sufficiently considered; and of the last portion, relating to the Armenian principalities, I have no more to say for my present purpose than what I have just said. I will now proceed to examine the Italian, South-Slavonic, and "Scythian" records, only considering historical questions so far as may be necessary in order to throw light upon the sources.

§ 8. The section on southern Italy (c. 27) seems to be based on informal inquiries made from Lombards, whose historical knowledge was inaccurate. One notices in this chapter the prominence which is given to Capua. It is exalted, in a certain way, among all the states of southern Italy. *πρῶτον δὲ κάστρον ὑπῆρχεν ἀρχαῖον καὶ μέγα ἡ Κάπυα* (p. 120, 20); Naples, Beneventum, Gaeta, Amalfi are enumerated after it without description. Again it is described as *πόλις ὑπερομεγέθης* (p. 121, 9), and the foundation of New Capua by Landolf is recorded. This points to Capuan informants, and it has been suggested that the information was gathered on the occasion of the embassy of Landolf who was sent to Constantinople by his father Atenolf, prince of Capua, in A. D. 909.<sup>3)</sup> In that case, the historical notices supplied by Landolf and his staff must have been registered at the time; and, when Constantine compiled this portion of his work in 948—9, he would have brought the chronological data into relation with the year in which he wrote.

1) *Chronicon Nestoris*, A. M. 6453 ed. Miklosich pp. 25, 29. The negotiations began before the deposition of Romanus who sent the first embassy. Cp. Dimitriu, op. cit. 545—9.

2) The notice of the Ezerites and Milingi (c. 50) is based partly on a chrysobull of Romanus I (223, 24) and an *ἀναφορά* of the strategos Johannes (222, 7). I suspect that the reference to the local *φύμη* of these tribes (221, 21) was recorded in the *ἀναφορά*. The acts of Krinites and Bardas must have been within the Emperor's memory; but his reference to the months of March and November (without mention of the year, p. 222) shows, I think, that he had a report of the acts of Krinites before him.

3) Cp. Jules Gay, *L'Italie méridionale et l'Empire byzantin* (1904) 170 note.

Plausible though this conjecture may seem, I cannot consider it certain. It is stated (121, 15) that New Capua was founded "73 years ago". The true date is c. 856, and Banduri thence concluded<sup>1)</sup> that the notice was written down in 929. If this were so, we should have to assume that the notice was copied in 949, without alteration of the no longer applicable date. If the other false date which appears in this chapter could be set right by referring it to 929, we could hardly escape from admitting Banduri's conclusion. It is stated that the division of Beneventum and Salerno was made 200 years before 948-9. This is exactly one hundred years out, and would have been even further from the truth in 929. Thus the key which would solve one difficulty fails for the other. 909 solves neither. The fact that the error as to the division of Beneventum and Salerno is *exactly* of 100 years, certainly suggests that 200 is an inadvertence for 100, and that not only was the passage written, but the information also received, in 948-9. I confess that in any case it seems to me highly probable that these records reached Constantine at that time from a Capuan source. This would not necessarily imply a Capuan informant at Constantinople. The information may have been gathered in Italy, and possibly in transmission the chronological errors might have arisen. His source is of course responsible for ascribing the partition of the Lombard duchies to Sicon and Sicard, who were dead at the time, instead of to Sikenolf and Radelchis.<sup>2)</sup>

There is in this chapter a point of considerable interest, the story of Narses and the distaff. Constantine has been reproached here for incredible chronological confusion. The Lombard conquest of Italy is connected with the reign of Irene, and it is she who is said to have sent the distaff and spindle to Narses. But the ridicule which falls upon the Emperor Constantine's pretensions as a historian must be somewhat mitigated when it is recognised that the story, as he tells it, was not a deposit of the reminiscences of his own reading, but was taken down directly from the mouth of a Lombard informant. We may smile at his naïveté in reproducing it gravely without a word of criticism, but the chronology is not his own. The story was evidently current among the *southern* Lombards; Beneventum is represented as the seat of the government of Narses. The legend, in its original form, was also obviously Italian, not Byzantine; our source is

1) P. 388 ed. Bonn. A Lombard embassy, Capuan or other, to Cple in 929 is not likely. For the prince of Capua invaded Apulia in 926 and Capua and Salerno were in open revolt up to 934. Cp. Liutprand, Legatio, 7; Gay, op. cit. 209.

2) Gay, op. cit. 62. Sicon and Sicard are also falsely described as brothers.

Paul, the Lombard historian.<sup>1)</sup> It was based on a motif which has given rise to other anecdotes about historical personages.<sup>2)</sup> It is generally when some event has made a strong impression on popular imagination, that the mythopoeic faculty, drawing from the well of folklore, invents stories of this kind. Such an event was the Lombard invasion and conquest of half Italy; and the story of the insulting message of Sophia to Narses and his invitation to the Lombards is a monument of the impression which the disaster made upon the south-Italians. The *south-Italians* I say advisedly; for that it was started in south Italy is suggested not only by the connexion of Narses with Beneventum in the later form of the story, as it is told in Constantine's treatise, but by his connexion with Naples in the original story. On the receipt of Sophia's message he withdraws from Ravenna to Naples: *itaque odio metuque exagitatus in Neapolim Campaniae civitatem secedens legatos mox ad Langobardorum gentem dirigit.*<sup>3)</sup> The withdrawal to Naples is historical; it comes from the *Liber Pontificalis*.<sup>4)</sup> It was here that he was said to have spun the fatal web, and here probably the story was set going.

It was remembered, and told from father to son, in the duchies of Naples and Beneventum; but we have still to consider how it came that the legend was modified in later times by the substitution of the Empress Irene for the Empress Sophia. Popular legend recks not of chronology; but though it may work blindly, it does not work without motives; and a motive there must have been. The clew for discovering it is given to us by the introduction of Pope Zacharias into Constantine's account: "In the times of the Empress Irene ὁ πατρίκιος Ναρσῆς ἐκράτει τὴν Βενεβενδὸν καὶ τὴν Παπίαν καὶ Ζαχαρίας ὁ πάπας Ἀθηναῖος ἐκράτει τὴν Ρώμην". It was in the pontificate of Zacharias that the Lombards conquered the Exarchate. That was an event which might well recall the original conquest, of which it might seem the consummation. This new conquest, this new defeat of the Empire by the Lombards, supplied, I believe, the motive for

1) Paulus, II 5. But the earliest source is Fredegarius, III 65 (p. 110 ed. Krusch).

2) Euelthon sent a distaff and wool to Pheretima of Cyrene, Herod. 4, 162; Hormisdas sent his general Varahran γυναικείας ἐσθῆτας, Theoph. Sim. 3, 8, 1. See my *Later Roman Empire II* 110, 146. It is remarkable that Sophia, not Justin, sends the emblems; does this point to the conclusion that the origin of this type of story was connected with the matriarchate? — The Emperor Manuel I punished an officer, responsible for the loss of a fortress, by exposing him in public in female attire on an ass's back, Kinnamos I 5.

3) Paulus, ib. 4) LXIII (Ioannes III).

chronologically remodelling, bringing up to date as it were, the old story. In the generation following that which witnessed the fall of the Exarchate, the tale of Narses and Sophia was applied to the event which was in recent memory; but for Sophia was substituted the contemporary Empress whose fame was in all men's mouths — the only Empress of the age whose name was familiar enough for the mythopœic instinct to fasten on. It mattered not at all that at the time of the conquest of the Exarchate, c. 750, she was only an infant at Athens, if she was even born. The name of Narses was kept; there was no one to take his place.

§ 9. There is another passage on south Italian history, which also contains legend, but it appears in another context, namely in connexion with Dalmatia: pp. 130, 15—136, 14 (in c. 29). It deals with the Saracen invaders of southern Italy, the expedition of the Emperor Lewis II, and the recovery of Bari; and its justification as a digression in the account of Dalmatia is that it follows on to the Saracen attacks on the Dalmatian coast and that the Slavs of Dalmatia performed military service for the Greeks in Apulia. It would however be equally, or rather more, in place in the south-Italian section (c. 25).

There are two serious (not to speak of minor) errors in Constantine's narrative. The capture of Bari by the Saracens, which occurred in A. D. 841<sup>1</sup>), is placed at the beginning of Basil's reign, just after the Saracens were forced to raise the siege of Ragusa; and two distinct sieges of Ragusa are confounded. Ragusa was attacked by the Saracens for the first time in 847—8 A. D., for the second in 866—7 A. D.<sup>2</sup>) It is with the second siege that Constantine is concerned. He names three leaders, the Sultan, Saba, and Kalphūs. Of these, the Sultan Mufareg ibn Salem, was doubtless the leader of the second attack; but the other two, Khalfun and Saba, almost certainly belong to the first attack.<sup>3</sup>) Constantine himself exhibits the falsity of his chronology by stating that after the capture of Bari the Saracens ruled over "all Logubardia for forty years" (130, 18). This

1) See the evidence in Hirsch, *Byz. Studien*, p. 255.

2) Cambridge Sicilian chronicle, ed. Cozza-Luzi, p. 28: *Ἐτος στρεψατησαν οι Πογοι — ινδ. ια',* p. 30 *Ἐτος στρεψατησαν οι Πογοι το β' ινδ. ια' ε'.* Gay (op. cit. p. 92) seems to contemplate the possibility that the first siege was previous to the capture of Bari, but we cannot set aside the date of the Sicilian annals.

3) Hirsch, ib. p. 255; Vasiljev, *Vizantia i Araby* (za vrem. mak. din.) p. 18, n. Khalfun took part in the capture of Bari in 841, Saba governed Taranto in 840.

number approximates to the truth; it is too large, if the period is reckoned to the recovery of Bari, too small if the conquests of Nicephorus Phocas are taken as the term.

It seems clear enough how the mistake arose. The author found in one source the fall of Bari and the (first) siege of Ragusa, noticed together. Assuming that this was the same siege which was raised on the arrival of Nicetas Ooryphas in A. D. 868, he fell into the further error as to the capture of Bari. The recovery of Bari is told briefly from the Byzantine point of view, with a suppression of the fact that it was Lewis who took possession of it and that it was in the hands of the Beneventans for some time before it passed into the power of the Basileus.

Then follows an account of the subsequent fortunes of Lewis II in south Italy. This is entirely of an anecdotal character, and the hero is the wily Sultan. The story of the Sultan never laughing is a variant (as Banduri pointed out) of the *διήγημα παλαιόν* about the captive of Sesostris which is told in Theophylactus Simocatta.<sup>1)</sup> The actual conduct of Adelchis and his party in excluding Lewis from Beneventum<sup>2)</sup> appears in the story as the effect of a crafty plot of the African. The whole narrative belongs to the same class of popular anecdote as the story of Narses; it was inspired by the interest which was aroused by the captivity of the African chief.

It is interesting to observe, as we observed in the Narses legend, how some historical events have the power of stirring popular imagination to array old stories in a new dress. The Saracen invasion of southern Italy had this power; and the story told by Constantine of the messenger who, captured by the sultan as he was besieging Beneventum, told the truth to the garrison, in spite of his captor's threats, is simply the old story of Sesualdus, the nutrarius Romualdi, who, according to the Lombard historian, acted in the same way at the siege of Beneventum by the Emperor Constans two hundred years before.<sup>3)</sup> This tale and the Narses legend are both Beneven-

1) VI 11.

2) Gay op. cit. p. 106. The story takes no account of the preceding captivity of Lewis, which inspired a popular Latin poem, printed in Muratori, *Ant. Ital.* II 711, and Du Méril, *Poésies populaires Latines antérieures au douzième siècle* (1843), 264—6. It seems impossible to explain the mysterious verse 'Exierunt Sado et Saducto, invocabant imperio' as containing a reference to the Sultan. The Latin sources have no mention of the tale that he instigated the plot. — For other errors in Constantine's narrative see Hirsch, 257—9.

3) Paulus, 5, 8. Hirsch calls attention to the resemblance, p. 259.

tane stories, a fact which is significant for the Lombard origin of Constantine's information. We saw that in the case of the latter and the other records in c. 27, Constantine's source was probably to be traced to Capua rather than to Beneventum itself. It seems significant that in the present case Capua and Beneventum are throughout mentioned as a pair, and Capua always first.<sup>1)</sup> Both c. 27 and c. 29 belong to the same year 948—9. We are probably justified therefore in assuming that they come from the same (a Capuan) source.

The passage under discussion has a special interest for our purpose, because we find a duplicate narrative in Constantine's *Vita Basilii*, and also a partial repetition in the treatise *περὶ θεμάτων*. We shall find a comparison of these three passages instructive.

[Adm. 128, 12—129, 2 = V. Bas. 288, 12—289, 2	
" 129, 2—19 = " 291, 1—292, 13	
" 130, 1—131, 12 = " 289, 2—290, 23, 292, 14—294, 2	
	= Them. II 61, 12—62, 18
" 131, 13—136, 10 = " 294, 3—297, 23].	

The account in Them. is short and summary; that in the *Vita* is of course "stylisiert" and verbally long. These sources show too noteworthy differences (not inconsistencies) from Adm. They both notice that the Ragusan embassy was sent to Michael III, but the envoys on reaching Cple found Basil on the throne. Adm. simply says *οἱ Ρρουσαῖοι ἐθηλοποίησαν Βασιλεῖφ*, not noticing that it was the moment of Basil's accession. Again while this work gives the exact duration of the siege of Ragusa, 15 months (130, 7), the other two accounts use *the same* general phrase *ἐπὶ χρόνον ἐποιώρχουν ἵκανόν* (Them. 61, 17 = *Vita* 289, 16).<sup>2)</sup>

In the portion which does not appear in Them., it may be noted that the *Vita* has additional points which are absent in Adm. We learn that the Sultan was a prisoner for *two years*<sup>3)</sup> at Capua (*Vita* 294, 7); and that the person who told Lewis that he had seen the prisoner laughing produced witnesses of the fact (294, 12). These differences show that the account in Adm. was not copied from the *Vita* nor vice versa.

The work on the Themes seems to be one of the earliest products of Constantine's literary activity, having been compiled before the end of 944 A. D., as we must infer from the way in which

1) 131, 13, 132, 1, 133, 18, 136, 11.

2) Further Them. and *Vita* both characterise with praise the general Nicetas; in Adm. only his office is mentioned.

3) The true time is 5 years, cp. Hirsch, p. 259.

Romanus Lecapenus is mentioned.<sup>1)</sup> It follows that the account here was not abbreviated from either Adm. or the Vita, but that all three were independently derived from the same document. There is one point in Them. which brings us into touch with the contemporary historian Genesios. The number of fortresses taken by the Saracens is said to have been 150, and this statement (also in Vita p. 292) is found in Genesios p. 62. The passages are parallel but not identical:

Them. 62: *παραλαβόντες πάντα τὰ κάστρα καὶ τὴν πᾶσαν Λογγι-βαρδίαν καὶ τὰ λοιπὰ κάστρα Κα-λαβρίας μέχρι Ράμης, ὡς εἶναι πάντα τὰ ὑπ' αὐτῶν πορθηθέντα κάστρα ρν'.* Gen. 116: *δι' ὅν ἐσπέρα πᾶσα καταδεδούλωτο, ἐν πόλεσι τῆς τε Σικελίας ἔτι μὴν καὶ Λογγιβαρδίας πολυαριθμήτους ἐπὶ τε καὶ ρ' τοῦ Γαλλεριανοῦ χωρίς.*

There is evidently a connexion here. Chronologically, Genesios might have consulted Them., for Genesios completed his work after 944 as is shown by his dedicatory verses (at the beginning of Book I) in which Constantine is addressed as *αὐτοκράτωρ*<sup>2)</sup>, while Them. as we have seen was composed before the deposition of Romanus. But the addition *τοῦ Γαλλεριανοῦ χωρίς* excludes this explanation. The solution must be that Genesios derived this record from a communication of the Emperor, at whose bidding (ὡς ἐκέλευσας, *προστεταγμένος* pp. 3, 4) he undertook his work.

Gallerianon (to digress for a moment) has proved a puzzle. "In den unteritalischen Quellen" says Hirsch<sup>3)</sup> "finde ich einen solchen Namen nicht genannt". But the interpretation is not difficult. Gallerianum, I have no doubt, was the name of the notorious and formidable stronghold of the Saracens on the Liris; and this passage of Genesios supplies us with the origin of the later name of the river, which, from that fortress, came to be called Garigliano = Galeriano, just as, for instance, Cagliari = Caralis.

We see at once that we are here getting a glimpse into Constantine's workshop. Author of these three accounts, he has drafted them independently at different times from a common document, which formed part of his collection of material. For the *περὶ Θεμάτων* he has abbreviated; for the Life of his grandfather, he has converted the

1) Pp. 36, 2, 54, 4. Cp. Rambaud, op. cit. pp. 164—5, where 934 is shown to be the terminus post quem.

2) And in his preface p. 4. It was a constitutional principle that while the *βασιλεία* was collegial, the *αὐτοκρατορία* was not. Genesios could not have designated Constantine as *δ αὐτοκράτωρ* while Romanus was regnant.

3) P. 169.

original into literary form; Adm., though it omits some details, evidently reproduces the original draft most closely. There is indeed one notable difference between Adm. and the Vita still to be mentioned. In the former the baptism of the Slavs is recorded before the attack on Ragusa<sup>1)</sup>, in the latter it is placed after the relief and is accounted for by the impression which the relief produced upon the Slavs. This motivation was evidently an afterthought of the Imperial writer, and would show that the Vita was composed subsequently to this part of our treatise.

For the date of the Vita Basilii<sup>2)</sup> it has been pointed out that our only evidence is the passage p. 282, where the capture of "Adata" in Constantine's reign is mentioned. Skylitzes records the capture of τὴν περιβόλητον Ἀδαπαν, by Bardas the Domestikos, but does not help us to a date.<sup>3)</sup> Adata, as Ramsay saw, is Hadath<sup>4)</sup>, and the date of its conquest has been supplied, only the other day, by a passage of the Arabic historian Yahya of Antioch<sup>5)</sup>, which has been translated for the first time by Vasiljev.<sup>6)</sup> From this source we learn that Leo, son of Bardas Phocas, took Hadath in A. H. 336 = A. D. 947 July 23 — 948 July 10. This gives us A. D. 948 as a limit post quem for the composition of the Vita Basilii. If I am right in supposing that the South-Italian passage in the Vita was written subsequently to the corresponding passage in Adm., which belongs to A. D. 948—9, we obtain A. D. 949—50 as the upper limit. This gives a period of ten years (to A. D. 959, † Constantine VII) for the Vita Basilii and the preceding 4 Books of the Continuation of Theophanes. I shall have

1) P. 129.

2) Professor Krumbacher will allow me to suggest, for the next edition, that these important works of Constantine deserve a few more lines in his G. B. L. to indicate the respective date limits of their composition.

3) Skylitzes-Cedrenus, II 136 (*Ἀδαπαν* error for *Ἀδαταν* so also 214). I do not understand how Rambaud (ib. 140) gets the date 956 from this passage, nor on what grounds he asserts that "Adapa" is right. *Ἀδαταν* is also preserved in Zonaras, 15, 9, 2 (ed. B.-W. p. 422), which (as Z. depends on Sk. here) enables us to correct the text of Cedrenus with certainty. Weil thought Adana was meant (followed by Finlay II 246 and Hirsch, Byz. Stud. 227), and would explain the corruptions τ and π for ν by the theory of an Arabic source (Gesch. der Chalifen II 473 n.).

4) Historical Geography of Asia Minor, 301: "a fort between Marash and Membitch".

5) The notices of Yahya bearing on the reign of Basil II were published and translated by Baron Rosen (Imperator Vasili Bolgaroboitea, 1883).

6) Op. cit., Prilozh. p. 65; chast I, p. 268. We are indebted here to Vasiljev for an important addition to our knowledge.

more to say on this question below (§ 14) in connexion with another duplicate passage.

§ 10. There can be no question that Constantine's information about the topography of Venice (c. 27) comes directly from a Venetian source. The general accuracy of his description of the islands and lidi, and its agreement with the descriptions in the *Chronicon Venetum* and *Johannes Diaconus*, have been shown by Kretschmayr.<sup>1)</sup> But it may be proved that the historical notices (c. 28) are also derived from Venice. They consist of two portions, the foundation of the place in the 5<sup>th</sup> cent., and the events of A. D. 809—10.

The account of the foundation (p. 123) reproduces obviously the Venetian living tradition. Attila is said to have laid Italy waste, including Rome and Calabria: popular tradition confused its reminiscences of the invasions of Attila and Alaric. The transition from this period to Pippin, son of Charles the Great, 350 years later, is marked by the characteristic vagueness which testifies to the nature of the record: *μετὰ δὲ τὸ ἀναχωρῆσαι τὸν Ἀτίλαν μετὰ χρόνους πολλοὺς πάλιν παρεγένετο Πιπίνος δὲ φῆξ.*

The narrative of the events of A. D. 809—10 displays still more clearly its Venetian origin. It exhibits the colouring with which the Venetians themselves would tell the episode at the end of the ninth or in the tenth century. Paulus, *Cefalaniae praefectus*<sup>2)</sup>, was present during the events with a fleet, but if his report had been preserved and used by Constantine, the account would have been different. It is true that the Venetians reply to Pippin *ἡμεῖς δοῦλοι θέλομεν εἶναι τοῦ βασιλέως Ρωμαίων καὶ οὐχὶ σοῦ*; but otherwise the rights of sovereignty which the Basileus admittedly possessed and exercised at that time are ignored. The submission of Venice to Pippin is represented as *εἰρηνικαὶ σπουδαί* between Venice and the king, whereas a valid treaty could only be concluded with the Basileus — as it was actually concluded by the negotiations between the two sovereign powers which resulted in the Treaty of Aachen (812 A. D.). For a criticism of the situation, it is sufficient to refer to the excellent dissertation of Lentz.<sup>3)</sup> Thus the story which Constantine naively adopts from a Venetian informant is less candid than the Venetian chronicles themselves. The fact that Venice was for some time in the hands of the Franks, and then, without being consulted, restored by the high con-

1) Die Beschreibung der venezianischen Inseln bei Konst. Porph., B. Z. XIII (1904) 482 sqq.

2) *Ann. regni Francorum*, s. a. 809, 810.

3) Das Verhältnis Venedigs zu Byzanz, 1. Teil 1891, pp. 31 sqq.

tracting parties, Charles the Great and Nicephorus (Michael I) to the Basileus, is passed over; and the subject state is represented as making a treaty, as if it were an independent sovereign power. The fact that a Roman Emperor could write this historical notice without a qualm is an eloquent illustration of the fullness of the practical independence of Venice in the tenth century.

In view of the constant relations of Venice with Constantinople, it would be vain to expect to specify the channel of Constantine's information. After the years 933—4, when a son of the Doge Pietro Candiano II was sent to the court of the Basileus, according to a custom of the time<sup>1)</sup>, down to the end of Constantine's reign, the Venetian chronicles do not specially record intercourse; but the constancy of intercourse is illustrated by the Venetian law of 960, which forbids Venetian subjects to carry letters from foreign lands (Germany, Italy &c) to the Basileus or any one at Constantinople, except official communications from the Doge's palace.<sup>2)</sup> Venice had been a sort of post office for Constantinople.

§ 11. The knowledge which Constantine shows of recent Italian history beyond the Byzantine sphere of action in the south, the information which he supplies about Hugo and Berengarius I (c. 26), have been brought into connexion with the marriage of his son Romanus to Bertha (Eudocia), Hugo's daughter, in A. D. 944. The lady was accompanied to Constantinople by Sigefridus, bishop of Parma, and Gay has observed: "ce texte si curieux nous montre comment les ambassadeurs italiens, Sigefrid de Parme et ses compagnons, dans leurs conversations avec Constantin l'ont informé des affaires d'Occident".<sup>3)</sup> But, I think, we must seriously consider an alternative possibility, that these historical notices were obtained from Liutprand, who arrived at Constantinople as ambassador of Berengarius II on Sept. 17 A. D. 949, and remained for more than six months.<sup>4)</sup> Though the death of Hugo (A. D. 947) is not mentioned, Lothaire is described as the present king; and the coincidence that the adjacent section on southern Italy had been written in A. D. 948—9, and that the loquacious bishop of Cremona therefore arrived when the Emperor was at work on his treatise, supports my conjecture that Liutprand was the informant.

But I can support this hypothesis by more particular positive arguments. Let us compare the main facts which Constantine records

1) Dandolo, Muratori, XII 201.

2) *Fontes rerum Austriacarum*, XII 19. Cp. Gfrörer, *Byzantinische Geschichten*, I 271—2.

3) Op. cit. p. 225. 4) *Antapodosis*, VI, 4 and 10.

concerning Lewis III, Berengarius, Rudolph, and Hugo, with Liutprand's *Antapadosis*.

Constantine	Liutprand
115, 12 Ἀδέλβερτον ὃς ἔγημε γυναικα τὴν μεγάλην Βέρταν καὶ ἐξ αὐτῆς τὸν προρρηθέντα φῆγα τὸν Οὐγωνα ἔτεκεν	I 39 Adalbertus — huic erat uxor nomine Bertha, Hugonis nostro post tempore regis mater.
115, 14—19 Lewis III comes to Italy; at Pavia; goes to Verona; is blinded. καὶ τότε ἐκράτησε Βεριγγέριος.	II 35; 38; 39; 41, et Berengarius regno potitur.
115, 21—116, 1, the Italians summon Rudolf from Burgundy.	II 60.
116, 1—5 War between Rudolf and Berengarius; B. at first successful, then R.	II 65, 66.
117, 11 Berengarius at Verona; slain by Φαλάμβερτος ὁ σύντεκνος αὐτοῦ.	II 68 (Flambertus quem sibi, quoniam ex sacrosancto fonte filium eius susceperebat, compatrem rex effecerat), 71.
117, 12 Rudolf reigns.	III 8.
117, 13—15 καὶ μετὰ τοῦτο ἐμήνυσεν δὲ λαὸς τῆς χώρας ὅλης εἰς Βεργωνίαν τῷ Οὐγωνι τῷ προρρηθέντι φῆγι, λέγοντες δὲ ἐλθε καὶ παραδίδομέν σοι τὴν χώραν.	III 12 consensu Lampertus archiepiscopus omnium Hugoni potentissimo et sapientissimo Provincialium comiti mandat ut in Italianum veniat regnumque Rodulfo auferat sibi potenter obtineat.
117, 16—20 Hugo arrives and Rudolf retires to Burgundy.	III 16.

I have set out this comparison to show the general agreement between the stories of the two writers — the brief sketch of the Emperor and the fuller history of the bishop. This general agreement would not be sufficient for my purpose, but there is one point which arrests the attention — the coincidence in the notice of Flambert's relation to Berengarius. But I have reserved two comparisons which seem to be almost decisive. In speaking of the struggle between Rudolf and Berengarius Constantine says:

116, 2: δέ μὲν ἡμισυς λαὸς ἦν μετὰ τοῦ Βεριγγέρι, δέ δὲ λοιπὸς μετὰ τοῦ Ροδούλφον.

Liutprand says the same thing in the same words:

II 65 unde factum est ut totius regni media populi pars Rodulfum, media Berengarium vellet.

It seems to me impossible to explain away this coincidence. Can we, for instance, consider it probable that Sigefrid of Parma, in speaking of the events of A. D. 922 at the court of Constantinople twenty two years later (A. D. 944), would have mentioned just this point which Liutprand emphasizes in his work?

Again compare the following passage:

Constantine 117, 20—118, 1 καὶ τελευτήσαντος αὐτοῦ [Ροδούλφου] ἀπῆλθεν Οὐγων — εἰς Βεργωνίαν καὶ τὴν γυναικα τοῦ Ροδούλφου ἦτις καὶ Βέρτα ὡνομάζετο ἔλαβεν εἰς γυναικα, τὴν δὲ θυγατέρα αὐτῆς ὀνόματι Ἀδέλεσαν δέδωκεν Λαθαρίῳ τῷ νίψ αὐτοῦ.	Liutprand IV 13. Burgundionum preterea rex Rodulfus mortem obiit; cuius viduam nomine Bertam rex Hugo — maritali sibi coniugio sotiarat. Sed et filio suo, regi Lothario, Rodulfi et ipsius Bertae natam nomine Adelegidam — coniugem tulit.
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Both writers record the three events, Rudolf's death, Hugo's marriage, Lothaire's marriage, in the same close connexion.

Now it is not too much to say that, if it had been chronologically (and otherwise) possible for Constantine to have consulted the *Antapodosis* (which was not begun till A. D. 958), these two comparisons, combined with the general agreement between the two documents, would be held sufficient to establish the conclusion that the *Antapodosis* was a source of Adm.; otherwise a common source would have to be assumed. I think, therefore, we need have little hesitation in concluding that it was Liutprand who on the occasion of his official visit to Constantinople in A. D. 949—50 (at which time we know that Constantine was engaged upon his treatise) supplied the Emperor with the notices which so remarkably concur with the story told in his *Antapodosis*.

And possibly there is a trace, in the *Antapodosis* itself, of Liutprand's reminiscences of his historical conversations in the Imperial palace. To his notice of the marriage of Lothaire (cited above), he adds the following observation: *quod Grecis omnibus non videtur idoneum, scilicet ut, si pater matrem, cum sint duo unum, uxorem accipiat, filius filiam non sine reatu valeat sibi coniugio copulare.* May not this remark, which has no bearing on the argument, have been prompted by Liutprand's recollection of an oral comment of the Emperor Constantine?

The data, then, seem to me to justify the conclusion that for this portion of the treatise *Liutprand was Constantine's informant.*

But there are some things in Constantine's narrative which are not to be found in Liutprand's book: (1) The coronation of Berengarius; (2) the story of Berengarius being taken for dead, p. 116, 5—13; (3) the subsequent relations of Rudolf and Berengarius, and how the latter frustrated the attack of the *τρεῖς μαρκήσιοι* from Burgundy. But these additions do not necessitate the assumption of a second source. If we can show that Liutprand was aware of any of these events, though he has chosen to omit them, we shall be warranted in concluding that, for these also, the Emperor's information was derived from conversations with him. Now he was aware of the expedition of the "three marquises" (Hugo, afterwards king, Boso, and the mysterious *Οὐγων δι Ταλιαφέρνουν*), for, though he does not record it in its proper place, he subsequently alludes to it. He says that Hugo et Berengarii iam nominati regis tempore cum multis in Italiam venerat; sed quia regnandi tempus ei nondum advenerat, a Berengario territus est atque fugatus.<sup>1)</sup>

*cum multis* corresponds to *μετὰ λαοῦ ἵκανον* (116, 23), and *territus* is a quite appropriate summary of the effect produced by the methods which, according to Constantine's account, Berengarius employed. We may, then, feel confident that the supplements to Liutprand contained in Adm. rest upon the authority of Liutprand himself.

§ 12. The cities of the Dalmatian Romanoi which formed the theme of Dalmatia do not properly belong to a section devoted to the *εθνη*. But it was convenient to treat them here on account of the close connexion of the subject with the neighbouring Slavs. There was no difficulty for the Emperor to obtain the topographical details which he gives about the cities<sup>2)</sup> through the strategos of Dalmatia. It is evident that the story of the Avar capture of Salona was a traditional tale at Spalato. Here especially and at Ragusa information was collected. The names of persons who migrated from Salona must have been preserved at Ragusa<sup>3)</sup>; and it is clear that the date assigned, A. D. 448—9, also comes from Ragusan tradition which falsely connected the event with the invasions of the great Attila. The description of each of the chief towns is of the same form: derivation of the name, indication of the nature of the site or size of the place, notice

1) *Antap.* III 12.

2) The theme included Zara, Traù, Spalato, Ragusa, Cattaro, and the Quarnero islands Veglia, Ossero, Arbe. Antivari belonged to the *θέμα Δυοράξιον*.

3) P. 137. In the list comes Valentinus father of Stephanus *protospatharios*. This (for the reading see Banduri's note p. 344) may be an anachronism, not a corruption.

of the saint who was specially revered, and description of the chief church or churches. In the case of Traù the last item is omitted, in the case of Spalato and Ragusa more information is given. But we can easily infer that this homogeneous information was all collected for the Emperor at the same time, and doubtless in 948—9.

We have seen that the accounts of the Croatians, Servians &c. (cc. 31—36) are closely connected with the account of the Dalmatian cities in c. 29 and must have been composed about the same time. It can be shown that information was here derived from Slavonic sources, but the natural place to obtain such knowledge was in the Dalmatian theme, and we shall hardly be wrong in inferring that it was collected there expressly by the Emperor's command. Jagić has touched on this question and conjectured that a strategos of Dalmatia may "gelegentlich in nähere Beziehungen zu einem von den vornehmern, vielleicht der herrschenden Familie angehörenden Kroaten getreten sein und sich von diesen über die Provenienz ihrer Herrschaft erzählen lassen".<sup>1)</sup> This is a just recognition of a Croatian source, but I am inclined to believe that the acquisition of the information was not such a matter of chance.

Of his Slavonic sources Constantine gives one explicit indication. In his notice of Martin the Frank he states that *λέγουσιν οἱ αὐτοὶ Χρωβάτοι θαύματα ἵκανα ποιῆσαι* (150, 4). This enables us to infer that the whole account of the peaceful policy of the Croatians, 149, 9—150, 16 is derived from Croatian statements. Nor is it I think at all open to doubt that the records of Constantine as to the association of Heraclius with early Croatian history (which has given so much matter for debate to modern inquirers) reproduce Croatian tradition. We have here the Croatian and Servian reconstruction of their own history, and the tradition started with the reign of Heraclius. The Croatians remembered that the father of Porga, and then Porga, were at that period their rulers. Earlier extant Greek sources contain no mention of the Croatians and Serbs in the seventh century, and this facts supports what the whole tenor of the context leads us to believe, that we have to do with Slavonic tradition. This tradition is in more than one respect demonstrably wrong, but it has a historical basis. The Slavonic occupation of Bosnia, Dalmatia, and Servia was prior to the reign of Heraclius and was effected by force, not through the cooperation of the Imperial government. But Constantine's records, though they misrepresent facts, show unquestionably that Heraclius

1) *Arch. f. slav. Phil.* 17 (1895) 359.

dealt in a more or less decisive way with the Slavonic question. The situation speaks for itself. Heraclius found the Slavs in possession of north-western Illyricum; he could not drive them out; accordingly he regularised their position; they recognised the formal authority of the Empire and became *δουλικῶς ὑποτεταγμένοι*. This has been generally recognised as the right inference<sup>1)</sup>), and it would not be necessary to dwell upon it if it had not been recently ignored in the masterly work of Jireček on the Romans of Dalmatia.<sup>2)</sup> He has failed here to appreciate the significance of Constantine's records. The "Eintritt geordneter Zustände", the "ruhige Verhältnisse" which existed, as he observes (p. 32), in the reign of Constans, presuppose a definite and formal understanding between the Slavs and the Roman government, and this pacification due to Heraclius (we are reminded of the pacification of the West-Goths by Theodosius I) was the historical motif of the Slavonic tradition which Constantine has preserved.

In this tradition the misrepresentation of the character (and date) of the "Landnahme" is intimately connected with the further

1) Compare Grot, Arch. f. slav. Phil. 5 (1881) 302 [this paper is an extract from a longer work, Izviestia Konstantina Bagr. o Serbakh i Khorbatakh, 1880 (St. P.)]; Oblak, ib. 18 (1896) 232 (where the consent of Rački, Rad 59, 202 is recorded).

2) Die Romanen in den Städten Dalmatiens während des Mittelalters, in Denkschr. der k. Ak. d. Wiss., 48 (1902) III, and 49 (1904) I and II. He places the Slavonic attacks on Dalmatia, the conquest of Salona &c in the reign of Phocas (I 26), and has called attention to an important neglected passage in John of Nikiu (Zotenberg, p. 343), which mentions Slavonic devastations of the Illyrian provinces, and an attack on Thessalonica, in the reign of Phocas. But he does not seem to have realised that the attack on Thessalonica, recorded in the Life of St. Demetrius by the Metropolitan Ioannes, occurred in the reign of Maurice, nor indeed to have distinguished the two Lives. For he writes: "Die Nachricht zeigt, daß die bisher angenommene Chronologie der Angriffe der Slaven und Avarn auf Thessalonich ganz unrichtig ist und daß die großen Kämpfe in die Zeit um 609 gehören, nicht in die Jahre 678—685, wohin sie Tafel u. A. verlegt haben". The motif of the Vita by Ioannes is the early attack under Maurice, the motif of the anonymous Vita is a later attack, more than sixty years after the first invasion of Slavs and Avars, and so probably in the early years of Constans (cp. Gelzer, Die Genesis d. byz. Themenfv., 49). The siege which John of Nikiu mentions under the 7<sup>th</sup> year of Phocas must be the same as that which the Metropolitan dates in the reign of Maurice; and the local authority must be preferred. Indeed, there need be no conflict of evidence, for the notice of the Ethiopic chronicle evidently sums up events which happened during a number of years ("on rapporte que les rois de ce temps détruisirent" &c). The record of John of Nikiu, therefore, *does not affect in any way* the second siege of Thessalonica, in the reign of Constans, recorded in the anonymous Vita.

misrepresentation which consists in ignoring the fact that the Slavonic settlers were at that time under the overlordship of the Avars. All the devastations are set down to the Avars; and when the lands are desolate the Croatians, Servians and their fellows suddenly appear "at the psychological moment", dropped as it were from heaven. Here we come to the unhistorical tradition, which stands in glaring contradiction to evident linguistic facts, that the Croats and Serbs migrated southward from lands to the north of Hungary. Jagić has ably set the historical facts in their true light, but I must dispute his criticism on the procedure of the Emperor Constantine.<sup>1)</sup> It is surely clear that the migration of these Slavs was an invention of their own, part of their reconstruction of their early history. White Servia and White Croatia are, says Jagić, a Phantasieland; but it was the phantasy of the Slavs, not of the Emperor or the Greeks, that was here at work. If the Slavs, as their own story was, received lands from the Emperor, not having been on the scene before, they at once had to confront the question, where did they come from? The existence of "Croatian" and "Serbian" tribes in the north supplied a motif for an answer. We have here to do not with combinations of a Greek author, but with a Slavonic construction of the past. Whether the invention was originally due to the Croatians or the Servians, the others followed suite. The Slavonic origin is shown not only by general considerations, but specially by the mention of the five Croatian brethren and two sisters in connexion with the alleged migration (143, 21). There is no evidence that the parallelism, on which Jagić insists, between the Servian and Croatian parts is due to Constantine's speculation. It was a necessary consequence of the adoption of a similar theory on the part of these peoples. Nor is there any evidence that the designation of the Zachlumi, Terbuniatae &c as Servian was the result of the author's logic, which starting with the politically defined Croatia assigned all the other Slavs of the area in question to the Servian ethnographical sphere. The conclusion rather is that the migration theory originated in Croatia, and that, as there was no ground for inventing a White Zachlumia and so on, the Zachlums and the others, not belonging to Croatia, were relegated to the White Servia.

All that we know of Constantine's procedure from the rest of the treatise shows that he was not in the least inclined to venture on such speculative combinations as those in which old Greek ethnographers like Poseidonios indulged. He simply collected and arranged

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1) Op. cit. 61.

information. And the greater part of his information in these chapters was obtained, to all seeming, from the Slavs through Dalmatian channels. For the later part of his Servian history (pp. 156—9), he had, without doubt, more direct and independent knowledge. The Imperial government was concerned in the relations of Servia and Bulgaria during the reigns of Leo VI and Romanus and must have been fully informed of the events here related.<sup>1)</sup>

We saw that c. 30 was compiled subsequently to cc. 29, 31—36 (above § 4). The account of the Avar capture of Salona is exactly the same narrative as that in 29. They supplement each other, as some small details absent in the one are found in the other.<sup>2)</sup> The author, it is clear, simply worked up twice the same document in his collection of Dalmatian material. It is obviously, as I said, a Spalato story. Jagić calls it "fantastisch" but admits that it has "einen gesunden Kern".<sup>3)</sup> Most stories of the kind have a kernel of genuine history. Apart from this repetition, the author had gathered new material since he wrote the Dalmatian portion of his work in 948—9. He repeats in 30 his notice of the Croatian conquest, but he adds the tradition of the five brethren and two sisters. He mentions the survival of an Avar remnant in Croatia, and the relations of the Croatians with the Franks. But his new matter is chiefly geographical. He gives the number and names of the Županates of Croatia, the geographical boundaries of Croatia, Servia, Zachlumia &c. Finally he notices the tribute paid by the Roman towns of the Dalmatian theme to the Slavs, here using an official source, a βασιλικὴ κείμενος of Basil I.<sup>4)</sup> It can hardly be doubted that the author had derived supplementary information from Dalmatia since 948—9. The difference in the source comes out in the notices of the baptism of the Croatians in 30 and 31.<sup>5)</sup> In 31 it is stated (148) that "Heraclius sent and

1) We may probably explain by a difference of source the contradiction between two statements as to the relations of Bulgaria with Croatia: 150, 20 διλλ' οὐδὲ Βούλγαρος ἀπῆλθε πρὸς πόλεμον κατὰ τὰν Χρωβάτων, εἰ μὴ Μιχαὴλ κτλ., and 158, 16 κατὰ τὸν καιρὸν οὐν ἐκεῖνον εἰσῆλθον οἱ αὐτὸι Βούλγαροι εἰς Χρωβάτων μετὰ τοῦ Ἀλογοβότουρον τοῦ πολεμῆσαι, καὶ ἐσφάγησαν πάντες ἐνεῖσε παρὰ τὰν Χρωβάτων.

2) The arrangement of the διλλάγιον, carefully explained in 29 (126, 16) is only alluded to in 30 (142, 11), and Kleisa (the kleisura) is not mentioned in 30; while 30 alone gives the number of the disguised Avars (143, 6).

3) Op. cit. 57. 4) 147, the exact sums are given.

5) Marquart, Osteuropäische u. ostasiatische Streifzüge (1908) p. XVIII, is right in identifying Porinos with Borna (Ann. r. Franc. s. a. 819). But he does not convince me that Porga is also the same, or that there was no actual foundation for the significance of the reign of Heraclius in Croatian history.

brought priests from Rome and had the Croatians baptised; and at that time their archon was Porga". In 30 they send to Rome of their own accord at a later period, after they had thrown off the yoke of the Franks, and ask for baptism, and the name of the archon was Porinus (145). Here we are in the ninth century. The two statements are of course reconcilable by assuming a superficial attempt to introduce Christianity in the seventh century and a complete falling away; but I have not to inquire here whether this explanation is right or the story of the earlier conversion was due to a wish to push back the origin of Christianity in Croatia as far as possible. We have only to observe here that when he wrote 31 Constantine knew nothing of the later baptism which he afterwards recorded in 30.

§ 13. I do not propose to enter here into a historical criticism of the difficult sections relating to the Hungarians and Patzinaks; but it is possible, without doing so, to point out a source from which the Emperor derived part of his information about the Hungarians.

In the reign of Romanus I, the Empire was twice invaded by the Hungarians, in April A. D. 934 and again in April A. D. 943; on both occasions peace was negotiated by the patrician Theophanes, the *παραχοιμώμενος* and the ablest minister of the time. The dates were recorded by the Logothete Symeon and are reproduced in the chronicles which depend upon his.<sup>1)</sup> On the second occasion, peace was made for five years, and secured by the sending of important Hungarian hostages to Constantinople.<sup>2)</sup> Now we must obviously bring into connexion with this the notice of Skylitzes<sup>3)</sup> that there was a cessation of the Hungarian inroads for a time when Bulusudes and "Gylas" came to Constantinople and were baptized and created Patricians. "Gylas" we are told remained a Christian and kept the peace, but Bulusudes was hypocritical in his acceptance of baptism and after his return home renewed the attacks upon the Empire.<sup>4)</sup> Skylitzes also mentions his fate after the battle of Augsburg. Bulusudes, as has generally been recognised, is identical with *Bouλτζούς* who is mentioned by Constantine (175, 14). Bultzu is doubtless the correct form

1) *Continuatio Georgii* pp. 913 and 917 (ed. Bonn) = *Pseudo-Leo*, 322, 325 = *Theodosius* pp. 281, 234 = *Pseudo-Symeon*, pp. 746, 748 = *Contin. Theoph.* pp. 422, 430.

2) *διμήρους τῶν ἐμφανῶν*, *Contin. Georg.* 917, *Contin. Th.* 431.

3) *Cedrenus* II 828 = *Zonaras* XVI 21, 14—18 (p. 484, ed. Büttner-Wobst).

4) Here belongs the expedition which suffered a severe defeat at the hands of Pothos Argyros, in the later half of Constantine's reign, but conjecturally before A. D. 955 (the year of Augsburg): *Cont. Th.* 462; *Pseudo-Sym.* 756.

of the name; Simon de Keza calls him Bulchu<sup>1)</sup>; and in other western sources he is called Pulszi<sup>2)</sup> and Bulgio.<sup>3)</sup> Constantine says that he was the son of Kalé, and that he held the dignity of *karchas*, — the third dignity in the Hungarian state, after the archon and the *gylas*. Skylitzes appears to have taken *gylas* for a proper name.

The visit to Constantinople of these two leading Magyars, the *gylas* and the *karchas*, has been placed by Krug between A. D. 943 and 948.<sup>4)</sup> But I do not think that we can probably identify them with the ὄμηροι τῶν ἐμφανῶν referred to by the Logothete Symeon. It would seem to be more likely that at or before the expiration of the Five Years' Peace, in A. D. 948, they came voluntarily to Constantinople, in order to arrange something more durable. (The passage of Skylitzes attests that the *gylas*, the more powerful of the two visitors, was in earnest.) And this inference is borne out by an important piece of information given by the Emperor Constantine, which has hitherto been most curiously misinterpreted. Dümmler says: "Aus dem Schweigen Constantins in dem Buche *De administr. imp.* über diese Tatsachen darf man schließen, daß sie nicht vor 950 stattfanden".<sup>5)</sup> But Constantine, though he is brief, is not silent. Here is what he says<sup>6)</sup>:

Ιστέον ὅτι ἐτελεύτησεν ὁ Τεβέλης, καὶ ἐστιν ὁ νίδος αὐτοῦ· ὁ Τερματζούς, ὁ ἀρτίως ἀνελθὼν φίλος μετὰ τοῦ Βουλτζοῦ τοῦ τρίτου δῆροντος καὶ καρχᾶ Τουρκίας.

Banduri renders as follows:

Tebelesque moriens filium reliquit Termatzum, qui nuper *in gratiam rediit cum* Bultzo tertio principe et carcha Turciae.

This version, which seems to have been widely accepted, ascribes to Constantine a statement which is neither Greek nor sense. The meaning is clear and unmistakable. ὁ ἀνελθὼν φίλος means "who came to Constantinople as a friend". It ought to be unnecessary to illustrate this signification of ἀνέρχομαι, but as it has been so strangely misinterpreted, I will quote *De thematibus* II p. 61, 19, τῶν δὲ εἰς τοῦτο ἀποσταλέντων ἀνελθόντων (from Ragusa), and, with Cple expressed, *Adm.* 118, 2 ἡ δὲ ἀνελθοῦσα ἐν Κπόλει.<sup>7)</sup>

1) P. 105 ed. Endlicher. The Notary of King Béla has *Bulsuu* (c. 55).

2) Ann. Sang. mai. 955. 3) *Gesta epp. Camerac.* I c. 75 (Pertz VII 428).

4) *Kritischer Versuch*, p. 263. 5) *Kaiser Otto der Große*, p. 495, n. 2. 6) 175, 11.

7) I see that Marczali (op. cit. p. 128) has translated correctly, „ki mostanában eljöve mint szövetségesünk Bultzúval együtt”, and I am pleased to find that he has drawn the same conclusion as I, that the information about the genealogy of the Arpadian family was derived from them (p. 97). — *Φίλος* is of course technical for an ambassador of a friendly power; see *De cerim.* II 15, p. 568, 585, &c.

We learn then from this passage that Bultzu came to Constantinople along with Termatzu, great grandson of Arpad. The inference seems to be that Termatzu is identical with the *gylas* of Skylitzes. Their coming was friendly, and *φιλος* is not the phrase Constantine would be likely to use if they came as hostages. That the date of their visit may have been c. 948—50, is suggested by *ἀρτιως*, taken in connexion with what we know of the date of the composition of the treatise (949—52). But the important point is that we can now point to the friendly visit of these two highly placed Hungarians as an opportunity which was utilised by the Emperor for obtaining information about their country. I would in particular trace to them the notices concerning the family of Arpad, and the offices of *γυλας* and *καρχας* (174, 20—175, 17).

The notice of the employment of the Hungarians by the Emperor Leo against Bulgaria (172, 15—173, 10) obviously depends on a Greek source. In this account it is stated that Liuntis (Leuente) son of Arpad (*τὸν Λιούντινα*) was ruler of the Hungarians. But when the sons of Arpad are afterwards named, as four in number, this name is not among them. I will not indulge in conjecture<sup>1</sup>), but only observe that critics, in considering this inconsistency, should take into account the fact that the two statements are derived from different sources.

The notice in c. 8 (p. 74) of the embassy of Gabriel to Hungary obviously depends on the ambassador's report. The name of the Basileus is not given; possibly it was either Romanus or Constantine himself; but it seems much more likely that it was Leo VI.<sup>2</sup>)

It is a not unimportant question whether the account of the early history of the Magyars, before they reached their ultimate home in Hungary, depends upon information obtained by the inquiries of Constantine himself, or upon information gained at an earlier period.

1) Cp. Kuun, Rel. Hung. II 5—8, and the suggestion of W. Pecz that Liuntis was the eldest son of Arpad and father of *Τασις* (B. Z. VI 587—8). It is possible however that the Greeks were mistaken in supposing L. to be a son of Arpad. The name is Levente; we meet a later Levente in a Hungarian chronicle (A magyar honf. kútf., 503). Lebedias is a different name; P. Gyula sees in it a distortion of Elend (ib. 895.) I question Marquart's interpretation (op. cit. 52 and 522) of Adm. 172, 13—21.

2) Because under Constantine VII peace with the Patzinaks had become a principle of state policy, as we know from Adm., and the actual application of this principle had begun in Constantine's minority, when the regent Empress used the Patzinaks, as Leo had used the Magyars, against Bulgaria (Georgii Cont. p. 879 ed. Bonn). — For the relations of the Patzinaks with Byzantium, Vasilievski in Zhurn. Min. Nar. Prosv., 164 (1872), Nov., Dec., Neumann B. Z. III (1894) 374 sqq.

Some light is thrown on this question by a comparison of two passages in which the boundaries of the "Turks" are described. They are not replicas as might at first sight appear.

## A.

P. 81 (c. 13). *ὅτι τοῖς Τούρκοις τὰ τοιαῦτα ἔθνη παράκεινται, πρὸς μὲν τὸ δυτικώτερον μέρος αὐτῶν ἡ Φραγγία, πρὸς δὲ τὸ βορειότερον οἱ Πατζινακῖται, καὶ πρὸς τὸ μεσημβρινὸν μέρος ἡ μεγάλη Μοραβία ἥτοι ἡ χώρα τοῦ Σφενδοπλόκου (ἥτις καὶ παντελῶς ἡφανίσθη παρὰ τῶν τοιούτων Τούρκων καὶ παρ' αὐτῶν κατεσχέθη). οἱ δὲ Χρωβάτοι πρὸς τὰ δρη τοῖς Τούρκοις παράκεινται.*

It is to be observed that B is in its proper place, in the description of the Hungarians in Section 3, whereas A is out of place in Section 1. B presents the boundaries of Hungary correctly, as they were in the time of Constantine — allowing for an error in orientation of about half a quadrant. We have the Bulgarians to the s. e., the Patzinaks to the n. e., the Franks to w. and n. w., the Croatians to the s. w. In A the omission of the "eastern" frontier has no significance (perhaps it is due to a copyist's parablepsia), but the designation of the "southern" boundary as Great Moravia is highly significant. The Great Moravia of Sviatopluk and his sons included Pannonia<sup>1</sup>), so that here the orientation is more distorted: "southern" practically represents western. But Great Moravia could not be described as a neighbour or boundary of Hungary after it had been conquered and occupied by the Hungarians. Therefore *this description (A) applies to Hungary before the conquest of Pannonia*, when the Magyars had not yet advanced further west than the land between the Danube and the Theiss. We have thus to do with a notice which dates from the reign of Leo VI, in the interval between the migration of the Hungarians from Atelkuzu and their occupation of Pannonia. The sentence *ἥτις καὶ παντελῶς — κατεσχέθη* is an addition of Constantine.

The importance of this result is that in the reign of Leo VI, before A. D. 906 (see below), information was obtained and recorded

## B.

P. 174 (c. 40). *πλησιάζοντι δὲ τοῖς Τούρκοις πρὸς μὲν τὸ ἀνατολικὸν μέρος οἱ Βούλγαροι, ἐν δὲ καὶ διαχωρίζει αὐτοὺς δὲ Τστρος —, πρὸς δὲ τὸ βόρειον οἱ Πατζινακῖται, πρὸς δὲ τὸ δυτικώτερον οἱ Φράγγοι, πρὸς δὲ τὸ μεσημβρινὸν οἱ Χρώβατοι.*

1) Cp. Schafarik, II 465. Otherwise Marquart, op. cit. 119.

about Hungary, whether from Hungarian ambassadors at Constantinople or by Greek ambassadors in Hungary. We have therefore ground for conjecturing that the account of early Magyar history in c. 38 may be derived from information obtained at the same time. We might even go so far as to speculate that the embassy of Gabriel, mentioned above, belongs to this time, and that his report was the source of Constantine's notices in c. 38 of the wanderings of the Magyars and Patzinaks. The object of his embassy was to incite the Magyars against the Patzinaks; so that it would have been particularly appropriate for him to learn all he could discover about their previous relations with each other.

A word must be said about the chronology, because it concerns the date of the section on the Patzinaks (c. 37). The migration of the Hungarians from Atelkuzu to Magyarország is causally connected with the war of the Empire with the Bulgarians. We have to do apparently with events of two years: (1) Symeon makes war on the Empire; the Hungarians join the Romans against Symeon, who is driven into making peace; (2) Symeon and the Patzinaks attack the Hungarians and force them to leave Atelkuzu. Unfortunately, the Byzantine chronicles supply no definite dates (apart from the worthless statement of Pseudo-Symeon).<sup>1)</sup> Nor can we depend upon the order of events as narrated in the chronicle of the Logothete and its derivatives. In these sources the Bulgarian war is noticed after the death of the Patriarch Stephanos, which occurred, we know otherwise, May 17 893.<sup>2)</sup> But on the other hand the elevation of Zautzas to the dignity of basileopator is recorded before the death of Stephanos, and de Boor has shown that it did not take place till 894.<sup>3)</sup> We cannot therefore rely upon the order, nor accept, without further consideration, the common date given for the outbreak of the Bulgarian war, 893 (so Finlay with reservation, Roesler, Jireček, Gelzer &c.).<sup>4)</sup> The only exact chronological statement we possess is that of the Annals of Fulda<sup>5)</sup>, where the alliance of the Romans and Magyars

1) Georgii Cont. p. 893 (ed. Bonn) = Theoph. Cont. p. 357. Hirsch (Byz. Stud.) does not discuss the chronology.

2) Hergenröther, Photius, II 697; De Boor, Vita Euthymii, p. 94.

3) *Ib.* 95—6.

4) Finlay, II 281; Roesler, Romänische Studien, 160; Jireček, Gesch. der Bulgaren, 163; Gelzer, *ap.* Krumbacher G. B. L.<sup>2</sup>, 977. It is to be remembered that Symeon cannot have ascended the Bulgarian throne before 893, or at least the end of 892; for in 892 Vladimir was still ruler (Ann. Fuld. *ad ann.*).

5) Pertz, I p. 412. The text is edited by Marczali in *A magyar honfoglalás kútfói*, p. 317.

against Bulgaria is placed in A. D. 896. This account has great importance, because it seems almost certain that it depends on information obtained from Byzantine envoys at the court of the Emperor Arnulf, and doubtless as Kuun has suggested, from bishop Lazarus whom Leo VI sent to Regensburg.<sup>1)</sup> The date given by these Annals is also supported by the independent testimony of the contemporary Arabic chronicler Tabari.<sup>2)</sup> We are therefore justified, so far as I can see, in placing the outbreak of the Bulgarian war and the Hungarian invasion of Bulgaria in 896, and the subsequent vengeance of Symeon and the Hungarian migration in 897 (or not earlier).<sup>3)</sup>

The overthrow of Sviatopluk's sons and occupation of Pannonia probably happened about 906 A. D. This is the approximate date to which the evidence points.<sup>4)</sup> Kuun has attempted to refer this event to 898, immediately after the migration from Atelkuzu and crossing of the Carpathians. But he misinterprets a passage in Adm. p. 176, where it is stated that the sons of Sviatopluk lived in peace for a year (*ἐνα χρόνον*), after which civil war broke out, and then *ἔλθόντες* of *Τούρκοι παντελῶς ἐξαλόθρευσαν*. He assumes that the destruction of Great Moravia occurred immediately after the outbreak of the dissensions; therefore in 896 (Sviatopluk died 894, a year of peace 895).<sup>5)</sup> But the passage will not bear this interpretation. It condenses the history of Moravia after the death of the great ruler — a year of peace, then discord until the catastrophe — and is inconsistent with the western Annals, which show that Great Moravia was still unconquered for the first few years of the tenth century.

1) Relationum Hungarorum — hist. antiquissima, II 28. See Ann. Fuld. ad ann. 896.

2) A. H. 283 (= 19. Feb. 896 — 7. Febr. 897). The passage is translated by Abicht, *Der Angriff der Bulgaren auf Kpel im Jahre 896*, Arch. f. slav. Phil. XVII 478 (1895), and in Russian by Vasiljev, *Vizantia i Araby za vremia makedonskoi dinastii*, Prilozh. p. 11. Vasiljev rejects this date, and places the peace with Bulgaria in 898 (ib. pp. 103 sqq.). But the coincidence of the eastern and western chroniclers, who are independent, is a strong argument for 896; especially as the western annalist's notice is probably derived from a Byzantine oral source. The views of Szabó, Hilferding, Drinov &c on this question are worthless.

3) I should not care to build much on the notice in the old Russian Chronicle under A. M. 6406 = A. D. 898, "the Hungarians passed by Kiev" (Nestor, ed. Mikl. p. 12), but so far as it goes it confirms the chronology deduced above.

4) See Dümmler, *Gesch. des ostfr. Reichs*, II 581. Cp. Dudik, *Gesch. Mährens*, I 352.

5) Kuun, op. cit. II p. 26. He thus seems to place the fall of Great Moravia before the migration from Atelkuzu which he assigns to 897, placing the occupation of Pannonia in 898 (p. 66).

Constantine (p. 164) states that the migration of the Patzinaks to Atelkuzu happened 55 years ago. He gives this date twice:

164, 11 πρὸ ἑτῶν δὲ πεντήκοντα <πέντε> οἱ λεγόμενοι Οὔξ κτλ.

164, 20 δεσπόζονσι τῆς τοιαύτης χώρας, ὡς εἰρηται, μέχρι τῆς σῆμερον ἔτη πεντήκοντα πέντε.

It is obvious that *πέντε* has fallen out in the first passage<sup>1)</sup>, and that the author had a precise date before him. Kuun has committed an extraordinary blunder in translating the Greek. He renders *ὡς εἰρηται* "uti fertur"! and comments thus: "Constantinus annos 55 commorationis Patzinacitarum in regione Atelkuzu haudquaquam affirmat, sed id solum refert quod dicitur: *ὡς εἰρηται* 'uti fertur', immo alio loco operis quinquaginta tantum annos Bissenos hanc terram habitasse dicit".<sup>2)</sup> Exactly the reverse is true. *ὡς εἰρηται*, "as has been mentioned above", proves that *πέντε* has fallen out in the first passage, and the repetition emphasizes the author's confidence in his date.

If then the date of the Patzinak occupation of Atelkuzu was 897 A. D., as our other data lead us to infer, the addition of 55 gives us 952 or, with inclusive reckoning, 951 A. D., as the date of the composition of the section on the Patzinaks. We obtain however the period 898—906 A. D., during which the Magyars were in possession of Eastern Hungary (between the Danube and Siebenbürgen) and had not yet taken Pannonia, as the time from which the notice as to the boundaries of the Turks in c. 13 dates, and perhaps also the information about their early history in c. 38. This time, while the fugitive Magyars were still stricken with terror at the name of the Patzinaks, seems the most likely date for the embassy of Gabriel.

As for the date of the composition of the Hungarian portion (cc. 38—40), we have no indication, beyond the limit given by the conjectural date (see above) of the visit of the gylas and karchas to Cple. But there is no reason for supposing that the Hungarian and Patzinak portions were composed at the same time. It is clear that the account of the Patzinaks was derived from a Patzinak source, that of the Hungarians from more than one Hungarian source. The difference of source is clear. (1) The occupation of the first home of the Hungarians (Lebedia) by the Patzinaks is not mentioned in the Patzinak

1) Marczali's hesitation is unnecessary (op. cit. p. 98). He translates the second passage (p. 115): "és azon uralkodnak ötvenöt én óta a mai napig", thus omitting altogether the important words *ὡς εἰρηται* — important because they demonstrate the textual error in the first passage, and thus dispose of Marczali's "nagy ellenmondás" (great contradiction).

2) Op. cit. p. 26.

story, but only in the Hungarian. (2) The second home of the Hungarians, which was possessed by the Patzinaks when Constantine wrote, is called Atelkuzu in the Hungarian story, but has no name in the Patzinak story. We can safely infer that the author was not himself working up common material about both peoples derived from one source, but reproduced Patzinak and Hungarian materials which had come to him independently of each other.

A word must be said about the notice of Great Moravia and the sons of Sviatopluk (c. 41), which the author has subjoined to this account of Hungary. Subjoined, I say; because, as the Moravian kingdom had ceased to exist for more than forty years, the notice of it had no independent value as practical information, and the justification of noticing it at all is that the subject is à propos of the Hungarians who overthrew the Moravian power. There is no indication of the source; but it may be suspected that we have here information which came to Constantinople shortly before or shortly after the catastrophe of Moravia. It is just worth conjecturing that the source might be connected with the embassy of Gabriel, which fell later than 898 and, as we saw, probably in the reign of Leo VI, while the Magyars were still pale with terror at the thought of the Patzinaks.

§ 14. The section which is numbered as c. 42 describes the geographical route from the Danube to the eastern coast of the Euxine. The text consists of two parts which must be distinguished. The description of the route is interrupted at 177, 20 by an account of the foundation of Sarkel, and is resumed at 179, 10. The story of Sarkel has clearly been inserted by the author in a document of different origin.

It is noteworthy that in this description the starting point is Thessalonica, not Constantinople. Two routes from Thessalonica to the Danube are implied: one to Belgrade, at the Hungarian frontier, a journey of eight leisurely days, which is not continued further; the other to Dristra at the Patzinak frontier. From Dristra the distance to Sarkel is given in days, and the distance to the Dnieper by the coast in miles (p. 179). The mileage of a number of other distances is recorded, and the description ends at Soteriopolis.<sup>1)</sup>

It is obvious that we have here to do with routes of commerce, and the chapter deserves to be commented on in that connexion. I will only point out that the conspicuous position here occupied by

1) For the geography of this chapter see Westberg, *Die Fragmente des Toparcha Goticus* (Zapiski of St. Petersburg Academy, ser. VIII, cl. hist.-phil., V. 2 1901) 94 *sqq.*

Thessalonica must be brought into relation with a change which occurred in the reign of Constantine's father and is assigned in the chronicle of the Logothete as a cause of the Tsar Symeon's declaration of war against the Empire. We are told that two Helladic merchants and Musikos a eunuch of Stylianos διέστησαν τὴν ἐν τῇ πόλει πραγματείαν τῶν Βουλγάρων ἐν Θεσσαλονίκῃ, κακῶς τοὺς Βουλγάρους διοικοῦντες ἐν τῷ κομμερχεύειν.<sup>1)</sup> The full bearings of this record require further elucidation; but in conjunction with the passage in Adm. it illustrates the position of Thessalonica in the trade of the Empire with the north.

The account of the foundation of Sarkel depends directly or indirectly on the report of Petronas, whom Theophilus appointed strategos of Cherson.<sup>2)</sup> Its interest for our purpose lies in the fact that there is a duplicate of it in the Continuation of Theophanes, *Vita Theophili*, c. 28; just as we found above (§ 9) a narrative common to our treatise and the *Vita Basilii*. Comparing the two texts, we find that the *Vita Th.* gives an almost exact literary version of the colloquial relation in Adm. We have here, it may be observed, an interesting object lesson as to the way in which a writer translated colloquial into literary (but unrhetorical) diction. *ἄσπρον δσπίτιον* (*ἄσπρο σπίτι*) becomes *λευκὸν οίκημα*, *καματερὰ καράβια* appears as *στρογγύλαις ναυσί*. The texts correspond closely (excepting one or two transpositions, due to the different contexts in which they occur).

I may point out a couple of textual errors in Adm.<sup>3)</sup> Of the garrison of Sarkel we read: *ἐν φ ταξεωται καθέζονται τὰ κατὰ χρόνον ἐναλλασσόμενοι*. Experience of old Greek texts has taught me the precariousness of numerical emendations, but here we must, without

1) *Cont. Georg.* p. 853. Cp. Finlay's observations, II 281.

2) The locality of Sarkel, and the circumstances of its foundation were discussed at the Archaeological Congress at Vilna in 1893, à propos of a paper read by Kh. I. Popov. See the report in *Viz. Vrem.* 1 (1894), 255—6. Sarkel had previously been the subject of a controversy between Vasilievski and Th. Uspenski in the *Zhurn. Min. Nar. Prosv.*, 265 and 266, 1889. See also Marquart, op. cit. 28.

3) Marczali (*Kütfai*, p. 132 n.) refers to the account in V. Th., but strangely makes no use of it for his text. His expression "boven elbeszéli" is misleading, as it suggests that V. Th. supplies more details than Adm. I notice that he reads *χλάνθια τ φ κατεπάνω Παφλαγονίας* without comment (translating "a paphlagoniai katapán hajón"). V. Th. has *κατ τοῦ κατεπάνω τῆς Παφλαγονίας*. What is said is that the fleet of Petronas consisted partly of the *βασιλικοπλόια* and partly of chelandia furnished by the Katepan of Paphlagonia. It is not stated that the latter accompanied them. Presumably *τῷ* is a misprint.

hesitation, for τὰ read τ'. The Vita Th. preserves the truth: 123, ταξ. καθ. τριακόσιοι κατὰ χρ. ἐναλλ. (2) 178, 2, ὁ γὰρ χαγάνος ἐκεῖνος ὁ καὶ πὲχ Χαζαρίας — ἀποστείλαντες — ἤτησαντο. If ὁ καὶ πὲχ were right, the singular ἀποστείλας ἤτησατο would be necessary. But the chagan and beg are different persons<sup>1)</sup>), and therefore we must restore καὶ ὁ πέχ. And this is what we find in Vita Th. 122, 19. (3) 178, 7, καὶ δὴ ὁ αὐτὸς Πετρωνᾶς τὴν Χερσῶνα καταλαβὼν τὰ μὲν χελάνδια εὑρεν ἐν Χερσῶνι. The sense shows that εὑρεν is corrupt. V. Th. τὰς μὲν μακρὰς νῆσας ἐκεῖσε πον προσορμίσας ἐπὶ τῆς χέρσου κατέλιπεν. Should we read ὄρμισεν<sup>2)</sup>?

Are we justified in concluding (with Hirsch<sup>6</sup>) that V. Th. is here directly derived from Adm.? We saw reasons for a different view in the case of the passage common to Adm. and V. Bas., and there is one particular point which suggests that here also we have to do with a similar relation. There is a small detail in V. Th. which is absent in Adm. The building of the fort is said to have been accomplished *μόγις μὲν* but *διὰ πολυχειρίας λαμπρῶς* (123, 12). This serves to confirm what we might otherwise conjecture, that the writer of the Vita Theophili had not the treatise of his master before him, but only a draft of the separate slip from which Constantine transcribed the passage in Adm.

In his admirable analysis of the Continuation of Theophanes, Hirsch determined the main facts about its composition. We are now in a position to illustrate the question further. The history of Geneios which was undertaken by Constantine's command failed to satisfy the Emperor, and he decided to have a better and fuller history compiled, under his own immediate supervision. He committed this work to an anonymous writer, but contributed to it one portion himself namely the Life of Basil, which is distinguished in style, and set apart by a particular preface, from the rest of the work. But the rest of the work, namely the reigns of the four preceding Emperors, could also claim to be in a manner his; he supplied and arranged materials for it, as the compilator expressly says: *τῶν καθ' ἔκαστα τὰς ὑποθέσεις φιλοκόνως συνέλεξε καὶ εὐσυνόπτως ἔξεθέτο* (Praef. p. 4). The writer goes so far as to describe himself as little more than an amanuensis: *χεῖρα μόνως λαβὼν ἡμῶν διακονουμένην σοι δοσα τοὺς πρὸ σοῦ βεβίσαι*.

1) See Ibn Fadhlân, in Kútöfi p. 217 where the king, khákán, is distinguished from the khákán bhú (= beg). Cp. Hunfalvy, Magyarország Ethnographiája, 209.

2) Marczali, ib., reads *évek* without comment, but translates as the sense requires "otthagyta a hajókat" = left the ships there.

3) Op. cit. 206.

The sources which Constantine's collaborator used consisted of Genesios and material supplied to him from the Emperor's collections. The narrative of Sarkel formed an item in these collections and was put to double use, the Emperor using it himself for Adm. and furnishing it to his collaborator for the *Vita Theophili*, just as he used the South-Italian narrative both for Adm. and for his *Vita Basilii*. Another case of such double use is the description of the signal fires, which appears in the Life of Michael III (c. 35) and in the treatise *Ad Romanum*, published as an appendix to the *De Cerimoniis* Book I<sup>1)</sup>, for here too we can hardly doubt that we have to do with a common draft.

We saw above (§ 9) that the upper limit for the *Vita Basilii* is A. D. 948. Now in an important passage, to which Hirsch called attention, in the Life of Michael II, there is a reference to Constantine's anxiety for the recovery of Crete<sup>2)</sup>, which Hirsch has rightly connected with the Cretan expedition under Gongyles in A. D. 949.<sup>3)</sup> We have thus reason to believe that the historical work, embracing the history of the Empire from the point where Theophanes broke off, was designed and already begun by A. D. 949; and this furnishes a terminus ad quem for the completion of the work of Genesios. It follows that the composition of the Continuation of Theophanes was going on under the Emperor's eye simultaneously with the compilation of the *De adm. imp.* It also follows that the design of the Continuation was not subsequent to the completion of the Emperor's *Vita Basilii*. The words of the compiler *ὅσα τοῖς πρὸ σοῦ βεβιωται* would not enable us to say whether in the original plan the *Vita Basilii* was to form a portion of the work. The special title of V. Bas. makes us rather think that its addition to the work was an afterthought (*ἢν Κωνσταντίνος βασιλεὺς τῷ γράφοντι προσανέθετο*<sup>4)</sup>), p. 211), and this, we shall see, is the true view.

We have no definite indication whether Constantine wrote the Life of his grand-father before or after the *completion* of Cont. Th. Book I—IV. I am here met by the fact that there are a number of passages in Book IV corresponding to passages in V. Bas., and considered by Hirsch to be derived from it.<sup>5)</sup> But here again, if I am not

1) Pp. 492—3. There is one touch in the *Vita*, which is absent in the App.: 198, 2 *ἐπεὶ κατὰ τὴν ἐσπέραν δὲ τοῦ Φάρου φανὸς διὰ τοῦ παππίον ἐδῆλον κτλ.* (App. 493, 15 *συνέβη τοὺς συνήθεις ἀφαι φανούς*). 2) c. 26, p. 81.

3) *εἰς Ἰνδικιῶνα* *ζ'*. *De Cerim.* II 45, p. 664. Hirsch, 180: gerade inmitten der Vorbereitungen zu diesem Zuge, kann man denken, hat der Kaiser diese Zeilen geschrieben.

4) *προσανέθετο* — contributed. Rambaud (187) mistranslates *dictée*.

5) Enumerated by Hirsch p. 222.

mistaken, we have to do with a double use of the Emperor's raw material. A comparison of the passages in question gives me the conviction that the writers of V. Mich. and V. Bas. have independently, each in his own way, worked up simpler statements, in colloquial language, prepared by the Emperor, — the former venturing on much less elaboration and periphrasis than the Emperor himself. The inference would be that while Constantine's nameless assistant was occupied in writing the history which was to supersede Genesios, Constantine, if he had not begun to compose the *Vita Basilii*, had at least conceived the intention of doing so and prepared material for the purpose, and, as some of this material was relevant to the latter part of Michael's reign, he furnished it for the use of his assistant. It is clear that, if the *Vita Basilii* had been designed to form part of the larger work, such reduplications would never have been admitted. They demonstrate that the Emperor's biography was joined on to the other work as an afterthought.<sup>1)</sup>

We observe that in the Continuation, Books I—V, there is no reference or allusion to any event subsequent to the Cretan expedition of A. D. 949, and this circumstance<sup>2)</sup> may well incline us to suspect that, though we cannot fix any formal terminus ante quem before the Emperor's death 959 A. D., the work is not later than Adm. The Life of Michael II was, as we saw, probably composed in A. D. 949, so that unless there was some remarkable interruption, Books I—IV must have been completed not later than A. D. 950. There is one fact which suggests that the *Vita Basilii* was also written then or not much later. It is very remarkable, and it struck Hirsch<sup>3)</sup>, that there is no mention in this work of Basil's conversion of the

1) And perhaps an afterthought several years subsequent to the completion of both works. Constantine's *ειζη*, I have no doubt, was to produce two distinct historical works, the one which he committed to his assistant and which embraced Leo V to Michael III, and a second on the Basilian dynasty coming down to his own time and to be written by his own hand. The first would serve as a foil to the second, the vices of Leo and the Phrygians to the virtues of the Armenian house. But he did not find leisure or opportunity to fulfil this hope, which he expresses in his preface to the *Vita Basilii* (p. 212); and when he abandoned the idea, he "contributed" this biography as an adjunct to the other history.

2) It is to be observed that the Continuer who added Book VI in the time of Nicephorus II or later did not introduce any interpolations into Books I—V. The passage in Book I (p. 21), in which Hirsch (p. 179) saw a reference to Nicephorus II, has been shown by Brooks to refer to Nicephorus I (B. Z. X 416). Thus there is no evidence of a Schlußredaktion of I—V.

3) Op. cit. 265.

heathen Greeks in the Peloponnesus — an achievement which is noticed in Adm. 224, 10. May we infer that this came to Constantine's knowledge after the completion of the *Vita*, which in that case was finished before the completion of Adm.? It is an argument from silence, and in itself inconclusive; but we may at least say that our data, so far as they go, suggest c. A. D. 949, 950 as the time at which the *Continuation* and the *Vita Basilii* were written.

§ 15. The only portion of the 4<sup>th</sup> Section which calls for special consideration here is the notice of the deliverance of Patrae from the Slavs in the reign of Nicephorus I (c. 49). The circumstances of the attack and the deliverance are described, the report thereof to the Emperor and his pronouncement are recorded, and his *σιγίλλιον* is referred to. Then the text proceeds:

*ταῦτα οἱ πρεσβύτεροι καὶ ἀρχαιότεροι ἀνήγγειλαν παραδόντες  
ἀγράφως χρόνῳ τε καὶ βίῳ τοῖς ὑστερον, δπως ἀν κατὰ τὸν προφή-  
την γνῷ γενεὰ ἡ ἐρχομένη κτλ. (219—20).*

Then it is stated that the burden was laid upon the Slavs to provide for the maintenance at Patrae of strategoi, basilikoi, and foreign envoys (*πάντας τοὺς ἐξ ἐθνῶν ἀποστελλομένους πρέσβεις*). This is interesting because it shows that ambassadors from southern Italy were in the habit of travelling to Cple via Patrae and Corinth. Finally the author refers to his father Leo's *σιγίλλιον* defining what dues the Slavs were bound to pay to the metropolitan of Patrae.

The narrative bears upon it the stamp of an ecclesiastical production of Patrae, composed — one infers from the passage cited above — in a generation subsequent to that which witnessed the siege. Both this passage, and the whole narrative, are marked by a tone different from that of the treatise, and show that we have here to do with a transcript (perhaps abbreviated). It is noteworthy that in these *Acta* the Greeks of the Peloponnesus are called *Γραικοί* (217, 8), — a usage which, we may infer, was current in the Peloponnesus, but probably not at Constantinople.

The character of this record as a transcript explains the absence of the normal introductory *ἰστέον ὅτι* (as mentioned above § 6). It must be left open whether the notice of the *σιγίλλιον* of Leo at the end was a part of the transcribed document.

§ 16. It will be useful to give a conspectus of the chronological results which have been established, or made probable, by this investigation, concerning both Adm. and other works composed by Constantine or undertaken by his command.

De Thematibus	.. 934 — end of 944
Genesius, <i>Βασιλεῖα</i>	.. 944 — 948
Continuatio Theophanis (I—IV)	.. c. 949—950
Vita Basili	.. c. 950 (prior limit, end of 948)
De adm. imp.	.. 948 July 15 — probably 952
Ad Romanum	.. 952—959

## De adm. imp.

948 Sept. 1 — 949 Aug. 31	.. cc. 27, 29, 31—36
949 Sept. — 950 end	.. c. 26
950 autumn —?	.. c. 30 (probably not before 951)
951 Sept. 1 — 952 Aug. 31	.. c. 45
952 (or 951)	.. c. 37.

§ 17. The defects of Constantine's treatise from a logical point of view are obvious. It deals with two different subjects, the *ξθνη* and internal administration; and this distinction should have constituted a primary division of the work. The first of these subjects falls again into two parts: diplomatic principles and historical descriptions. Constantine has not only coordinated these divisions and subdivisions, but has even broken up the diplomatic portion, quite unnecessarily, into two further subdivisions (Sections 1 and 2) and coordinated these also. The logical division would be as follows:

Part I: the *ξθνη*: (1) diplomatic relations (= Sects. 1 and 2);  
 (2) historical and geographical information  
 (= Sect. 3).

Part II: facts about internal administration (= Sect. 4).

In the second place, the Emperor has placed some of his notices in the wrong place. Thus the account of the route of the Russians from Kiev to Cple has no right to be in Sect. 1 where it occurs (c. 9), but ought to come in Sect. 3; and the same remark applies to the boundaries of Hungary (c. 13) which are clearly out of place. Here too belongs the position which Constantine assigns to Imperial Dalmatia. The Dalmatian towns, Ragusa, Zara, Cattaro, Traù &c formed a *στρατηγία*, and were therefore exactly on the same footing as the theme of Cherson. As an integral part of the Empire they ought not to be treated among the *ξθνη*. It was inconsistent to deal with Cherson in Sect. 4, and the Dalmatian theme in Sect. 3. Of course, one can see how this happened; the close connexion of the Roman Dalmatians with the Croatians made it convenient.

The natural arrangement of the author's 3<sup>rd</sup> Sect. was geographical, and this he adopted. He begins in the east with the Caliphate, and

comes round to the east again, by Spain, Italy, northern Europe, to Armenia. But he spoils this arrangement somewhat by seeming to divide it into two parts northern and eastern, designating the Armenian portion as the eastern. It would not be fair to say that he is thereby committed to including the Caliphates and Italy in the northern, for he only distinguishes the Armenian portion from the account of the Scythians of the north (p. 182); but logically he ought to have indicated corresponding divisions between (1) the Caliphates and Italy, and (2) the Croatians + Servians and the Patzinaks.

Another criticism which we are justified in passing on Constantine's work relates to its remarkable omissions in his account of the *εθνη*. We are entitled to expect historical and geographical accounts of Germany, Bulgaria, Khazaria, and Russia. Russia indeed has been to a certain extent provided for in the notice which, as we saw, is out of its place; but a great deal more information about the Russian state was certainly accessible to Constantine; the recent treaty of A. D. 945 had been concluded under his own *αὐτοκρατορία*. To Germany there are only a few incidental allusions; yet here information was equally accessible. The court of Otto had been visited by a Greek embassy in 945; and Salomon, a chitonites, was sent again in 949 with rich presents and returned to Cple in the same year accompanied by Liutfred a merchant of Mainz, whom Otto selected as his envoy.<sup>1)</sup> Thus at the very time when the Emperor was engaged on his work and was collecting Italian information from Liutprand, he had excellent opportunities for informing himself about Germany. Stranger still is the omission of Bulgaria. He has indeed occasion to notice episodes of Bulgarian history, but these notices are always incidental, à propos for instance of the Hungarians and the Servians. But it is extraordinary that he should sketch the history of Servia and not that of Bulgaria. The Khazars are treated in the same way; they too only come in incidentally, in connexion with the Patzinaks, the Hungarians, and the route from the Danube to the Caucasus. No motive can be assigned for these omissions<sup>2)</sup>; it is impossible to conceive that Constantine deliberately intended to exclude these peoples from his *εθνογραφία*; and we are almost forced to conclude that Constantine set aside the work in an unfinished state and never completed it.

1) Liutprand, *Ant.* VI, 4; *Ann. Quedl.* sub. a. 949.

2) The list could be increased. Relations with Ethiopia and South-Arabia in Constantine's reign can be inferred from *De Cer.* II 48 p. 691. And we expect notices of such dependencies as Sardinia, and the Illyrian "Moravia" (see *ib.* 690, 691).

We have already, in discussing c. 30 (§ 4) seen traces of want of revision.

This inference is, I think supported by the character of the third subdivision of Section 3. It is introduced as dealing with  $\tau\alpha\ \xi\nu\ \tau\iota\sigma\iota\ \kappa\iota\varphi\iota\sigma\iota\ \mu\iota\tau\alpha\kappa\nu\ \mathcal{P}\omega\mu\alpha\iota\omega\ n$  καὶ διαφόρων ἐθνῶν συμβεβηκότων, but only one instance is given, the negotiation of Justinian II with the caliph περὶ τῆς τῶν Κυπρίων μεταναστάσεως. Parturiunt montes! But the inconsistency is positive and formal. The words  $\xi\nu\ \tau\iota\sigma\iota\ \kappa\iota\varphi\iota\sigma\iota$  and διαφόρων ἐθνῶν distinctly show that the author contemplated several cases, and prove that he laid down the work before he had completed his design.

The value of the treatise, disfigured though it is by logical defects of arrangement and by some historical errors (such as representing Leo IV as *husband* of a Khazar princess), is incontestable. It illustrates the remark of Krumbacher about Byzantine historians generally: "So weit es die persönlichen Kräfte und die Bedingungen des Zeitalters gestatten, streben die Geschichtschreiber in Byzanz nach Information und bemühen sich von wohlunterrichteten Personen ausführliche Nachrichten zu erhalten".<sup>1)</sup> The account of Venice, which can be tested in detail, comes on the whole triumphantly out of the ordeal. It is obvious that the author spent great pains in gathering particular information from Dalmatia, for the purpose of including it in his treatise, and this portion specially exhibits his love of facts and details. His most serious mistakes are due either to the confusion of two similar events (as in the chronology of the Saracen capture of Bari)<sup>2)</sup>, or to repeating popular tradition as if it were historically accurate (as in the case of Narses). But, to quote Krumbacher again on the historians: "daß sie hinter der modernen Genauigkeit weit zurückbleiben, ist kein Vorwurf".

Our investigation has enabled us to win a closer view than before of the Emperor's literary activity in the sphere of history. We have learned that it was in the years 949—52 that he was specially pre-occupied with this subject, that he was engaged at the same time in composing the *De adm. imp.*, in writing the Life of Basil, and in supervising the composition of the Continuation of the Chronography of Theophanes. He had collected since before 945, and continued to

1) G. B. L. 229.

2) It is always worth remembering that in the nineteenth century also scholars of no mean capacity have been guilty of just as serious blunders. For instance Muralt placed Pippin's siege of Venice (810) in 754, through confounding Pippin the son, with Pippin the father, of Charles the Great (*Essai de chron. byz.*, 356).

collect, various material, which he used not only for his own works, but also to assist those who wrote by his command. He does not copy from one book into another, but when he has to deal with the same subject in different books, he works it up each time independently from the "slips" or notebooks of his collection.

There is a great deal to be done still for *De administrando imperio*. We want above all a new critical text, and a commentary abreast of recent research. The manuscript material is so limited that to supply the former would be a comparatively light task, but a historical commentary would be a formidable undertaking. Meanwhile I venture to hope that this study of the treatise may prove of some service.

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